HARVARD
PSYCHOLOGICAL STUDIES

EDITED BY

HERBERT SIDNEY LANGFELD

VOLUME V

CAMBRIDGE
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PREFACE

The fourth volume of the Harvard Psychological Studies, edited by Hugo Münsterberg, appeared in 1915. The death of Professor Münsterberg who had been Director of the Psychological Laboratory since 1892 occurred on December 13, 1916. The plan of publication which he adopted has been followed in the present volume. It is a collection of articles and monographs descriptive of experimental research carried on by members of the Department and graduate students and published in the various scientific journals. Unfortunately it has not been possible to publish all of the investigations completed since 1914. The researches in comparative psychology were conducted by Professor R. M. Yerkes and his students. The investigations in physiological optics were performed by Dr. L. T. Troland. The remaining experiments were made under the supervision of Professor Münsterberg and after his death under that of the present editor with the coöperation of Dr. F. H. Allport.

H. S. L.

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THE INFLUENCE OF THE GROUP UPON ASSOCIATION AND THOUGHT

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INTRODUCTION

If social psychology is to achieve the title of an independent science, it is high time that its many speculative theories and crude generalizations be subjected to experimental methods. The data of this science, it appears to the writer, may be for convenience subsumed under two heads, viz.: (1) the behavior of an individual in direct response to social stimulus, that is in response to some form of behavior in others, and, (2) behavior which is the response to a non-social stimulus, *e.g.*, a column of figures to be added, or a meal to be eaten, when such response is modified by the presence and actions of other persons. Responses to direct and incidental social stimuli are, in brief, the two classes of data for social psychology.

The following experiments bear upon certain problems of the second class of data mentioned. The method employed was to compare the mental processes (in this case association and thought) of the individual when alone with his reactions to similar and equivalent stimuli when a member of a "co-working or co-feeling" group. In this manner the part played by incidental or contributory social stimulation was determined.[1]

[1] A brief historical account of the study of the influence of the group upon the individual may be found in an article by W. H. Burnham: 'The Group as a Stimulus to Mental Activity,' *Science*, N.S., 1910, Vol. 31, 761–767.

General Method.—It was considered advisable to eliminate all incentives to rivalry which were not inherent in the very nature of the situation (*i.e.*, individuals working on similar tasks in one another's presence). The subjects were instructed not to regard their work as competitive; overt comparisons between individuals were also prohibited. The time given for the tests was constant, hence no one subject finished before the others. In this way rivalry, which is a distinct social problem and which should be studied separately, was reduced to its natural minimum. Each subject, however, was instructed to acquire the attitude of doing his best in both the group and the solitary work.

The subjects were arranged in groups, containing from 3 to 5 subjects each. The groups had no changes of personnel during a whole experiment. The subjects were upper classmen and graduate students in psychology at Harvard and Radcliffe Colleges. They were 26 in number, though not more than 15 were used in any single experiment. There were 24 men and 2 women. In age they ranged from 20 to 40 years, 26 being the average age.

In the group work the subjects sere seated one on each side of a table 3 feet by 5 feet in dimensions. In groups of 5 two subjects sat at one of the longer sides. The same seats were retained by subjects throughout the course of an experiment. Care was taken to secure conditions, such as type of table, light, air, seating of the subjects, etc., in the rooms used for solitary work comparable to those conditions in the room where the subjects worked as a group.

The free chain associations which were to be written were started by a stimulus word, for example 'building' or 'laboratory,' written at the top of a sheet of paper given to each subject. The same stimulus words were employed in the two conditions, *T* and *A*.[1] It was also emphasized in group work that the same stimulus word was given to all. It is not believed that the presence of the experimenter in the group work materially affected the results of the social influence.

In all experiments except the first constant intervals of

[1] *I.e.*, 'Together' and 'Alone.'

time were given, in the group by spoken signal, and alone by buzzers placed in each room and tuned down to inobtrusive intensity. Control tests were given in the group, using the buzzer for signals in order to determine whether the buzzer itself played a part in the results. No difference was found in the average, between group tests given by the buzzer and those given by verbal signal. The writing materials (pen, pencil, etc.) used by each subject were kept as constant as possible throughout the experiment.

Experiment I

Free Chain Association

1. *Procedure.*—The first experiment, introductory in character, was the only one in which the amount of work was constant, and the time required to finish was taken as the objective result for each subject. Sheets of paper were ruled for writing 100 words. Three tests were given within the hour. Only one group, consisting of but 3 subjects, was used for the experiment. The experimental hour came once per week, and the experiment lasted about 12 weeks. There were weekly alternations of the social conditions, together (*T*) and alone (*A*).

The papers were placed before the subjects face down. At the signal they were reversed and the subjects glanced at the word given at the top and proceeded to write their free associations one below the other. The writing of the successive words of sentences or phrases was prohibited as was also the serial association of numbers. While working together the time for each subject was taken by the experimenter; when working alone each subject timed himself with a watch or stopwatch. A rest of three minutes was given between test sheets. Immediately after the completion of each test, both together and alone, the subject was required to mark with distinguishing symbols certain kinds of ideas, as follows: (1) *Personal associations* (ego-centric), that is, words related directly and intimately with the subject's own past as experience not likely to play the same part in another's association trends; and (2) words which were written without

discoverable connection with any of the preceding words, that is, 'free-rising ideas.'

Treatment of Data.—In the following pages the term 'social increment' is used to indicate a gain in the average quantity of work done in the group over the average done alone. 'Social decrement' indicates a loss in quantity in the group performance. Corresponding gains and losses in *quality* of the work in the group are termed 'social supervaluents' and 'social subvaluents' respectively. The social increments, decrements, super- and subvaluents, are always expressed as a percentage of the average quantity or quality of the work done alone.) In this experiment the individual social increments or decrements are given on the right in Table I.

The results expressing the social influence upon the kind of associations (personal, objective, etc.) were too meager to admit conclusions. They are therefore omitted for this first experiment.

TABLE I

AVERAGE TIME SCORE FOR 100 WORDS
(No. of trials for each individual: Alone 9, Together 12)

Subject	Alone	Together		
Bar	4.3	3.9	Gain together	9.3%
Stu	5.8	5.0	Gain together	13.8%
Lan	3.3	3.4	Gain alone	3.0%
Average	4.4	4.1		

3. *Discussion of Results.*—Two out of three subjects have a social increment. Both of these increments were much greater than the social decrement of the other subject. For the group there was an average gain in time over solitary work of three tenths of a minute. The first rough indications therefore point toward an increased number of free associations produced in the group.

EXPERIMENT II

Free Chain Association

1. *Procedure.*—The study of free associations under the social influence was now continued using a larger number of subjects and tests. The groups used all came twice within a

week, the experiment lasting three weeks. There were 15 subjects arranged in 3 groups of 5 subjects each. It was decided to use fairly frequent alternations of the conditions *T* and *A*, thus equalizing effects of practice. One group (*C*) underwent alternations of "together" and "alone" *on the same day:* sometimes two and sometimes four alternations. A combination was used of the sequences *T A T A* (or *A T A T*) and *A A T T*. If the tests of a certain day began with *T* and ended with *A*, those of the following day would begin with *A* and end with *T*. The entire series moreover began with *T* and ended with *A*. Hence the initial lowering due to newness of the task and the final possible increase due to practice would favorably affect only the score for work done *alone*. The other two groups (*A* and *B*) changed their social condition on successive days. Group *A* began with a day in *T* and ended the series with *A*; group *B*, as a check, began the series with *A* and ended with *T*.

The routine of this experiment had one important difference from that of the preceding. The *time* was now the constant factor, and the *number of words written* was the measure of the association process. The interval of each test was three minutes, given with a suitable preparatory interval, by voice in the groups, and by buzzer when the subjects worked alone in separate rooms. A short rest period was allowed between tests.

Another important variation was the division of the work of each test into three *periods* of *one minute each*. After one minute of the time had elapsed the experimenter (in the groups) directed "draw line," whereupon each subject quickly made a line under the word he was then writing or had just written, and then continued with his work. This was repeated at the expiration of the second minute. When the subjects worked alone these signals for the divisions were given by short strokes of the buzzer. The rooms of the subjects when working alone were interchanged from day to day in order to obviate the effect of the peculiarities of any one room upon the work of a subject.

To the two types of associations required to be marked in experiment I. were added two more, viz: (1) words, other

than the first, suggested mainly by the stimulus word; and (2) words suggested by the immediate surroundings. Introspection was required immediately after each test together with a rough estimation of the degree of the rivalry consciousness expressed on a scale of 0 to IV. In all other respects not mentioned the procedure was the same as that in experiment I.

2. *Treatment of Data.*—The individual tables are omitted because of lack of space. Table II. presents the individual averages. Practice effects at the beginning which appeared in the individual records have been eliminated from the averages by the following rule. In the first day's work (group *A* and *B*) all tests which are lower than every single score made in tests on later days are ruled out.

TABLE II

INDIVIDUAL AVERAGES OF ASSOCIATIONS

Subject	No. Trials[1]		Alone				Together			
			No. Asso. per Min.			Total No. Assoc.	No. Assoc. per Min.			Total No. Assoc.
	A	*T*	1st. Min.	2d. Min.	3d. Min.		1st. Min.	2d. Min.	3d. Min.	
And.......	13	15	18.3	18.3	19.3	56.	20.5	18.7	19.	58.3
App.......	11	10	21.5	20.8	20.3	62.7	23.6	20.2	19.6	63.4
Cot.......	5	6	23.8	22.4	21.6	67.8	28.1	25.8	24.6	78.7
Hor.......	11	10	19.3	18.2	16.6	54.3	20.3	16.9	17.2	54.4
Hos.......	6	6	15.1	14.1	13.5	44.2	16.5	15.6	16.1	48.3
Hun.......	13	16	18.7	18.9	19.	56.8	19.8	16.8	18.4	55.1
Kno.......	14	15	18.7	18.5	20.	57.3	20.	20.4	17.6	58.2
Lan.......	13	15	19.3	18.8	18.9	56.8	20.2	20.2	20.7	61.2
Pep.......	13	12	24.2	21.9	22.2	68.4	24.2	23.5	23.5	71.2
Pre.......	14	16	24.9	22.2	22.7	69.9	26.4	23.7	22.2	72.4
Rob.......	12	13	22.	21.6	21.3	65.	24.1	24.3	24.6	73.
Spe.......	14	15	22.3	19.2	25.7	67.4	22.2	23.	22.2	67.5
Sto.......	14	16	15.2	12.5	12.3	40.1	16.5	15.	15.1	46.5
Tul.......	13	12	27.5	25.	24.3	76.9	28.2	26.8	25.7	80.9
Woo.......	5	10	21.8	20.6	19.	61.4	22.8	21.4	21.5	65.7
Average ..	11.4	13.5	20.8	19.5	19.8	60.3	22.2	20.8	20.5	63.6

3. *Discussion of Results.* (*a*) *Quantity of Associations.*—Table III. presents the average number of associations, together and alone, for each subject, together with the per cent. of gain under the social condition to which it belongs. We find our first experiment amply verified. 93 per cent.

[1] Exclusive of the trials eliminated owing to effect of practice.

of the subjects (14 out of 15) produce more associations in the group than they produce alone. The social increments are not large, but their preponderance is conclusive. Cut, Rob, and Sto have considerable social increments (from 12 per cent. to 16 per cent.). The average social increment also of the 14 is twice as great as the social decrement in the case of the one exception to the favorable group influence, Hun. The number of associations produced by all subjects together is also slightly greater than their average alone (63.6 to 60.3).

TABLE III

PERCENTILE GAINS IN AVERAGE NUMBER OF ASSOCIATIONS

Subject	Ave. No. Associations		Per Cent. of Gain	
	Alone	Together	Alone	Together
And	56.	58.3		4.1
App	62.7	63.4		1.1
Cut	67.8	78.7		16.
Hor	54.3	54.4		.2
Hos	44.8	48.3		7.8
Hun	56.8	55.1	3.	
Kno	57.3	58.2		1.5
Lan	56.8	61.2		7.8
Pep	68.4	71.2		4.
Pre	69.9	72.4		3.7
Rob	65.	73.		12.3
Spe	67.4	67.5		.1
Sto	40.1	46.7		16.4
Tul	76.9	80.9		5.2
Woo	61.4	65.7		7.
Average	60.3	63.6	3.	6.2
Mean variation	7.6	8.5	—	4.1

Number of subjects having higher average number of associations together....... 14
Number of subjects having higher average number of associations alone......... 1
Number of subjects having equal average number of associations together and alone 0

The mean variation among the subjects is higher, relatively to its mean, in group work than in solitary (8.5 compared with 7.6). Hence we find increased variability accompanying a social influence toward increased mental activity.

Let us now consider the *distribution of the social increment.* The question proposed is whether the increase due to the presence of the group was equally distributed, or whether it occurred chiefly at the beginning, the middle, or the end of the three-minute period. The record of each subject in the

three minutes is shown in Table II. We may compare the results of the three-minute periods both in number of subjects who have social increments in those minutes respectively, and also in the percentile value of the gain in group work shown in the average of all subjects in the three minutes respectively. This latter comparison is taken from the averages at the foot of Table II. We may speak of this gain as the 'group social increment.' Table IV. presents the above relations.

TABLE IV

		1st. Min.	2d. Min.	3d. Min.
No. of subjects having greater number of associations *together*		13	12	9
No. of subjects having greater number of associations *alone*		1	3	6
No. of subjects having equal number of associations *T* and *A*		1	0	0
Group Social Increment	Amount of excess of Together over Alone	1.4	1.3	.7
	Per cent. of excess of Together over Alone	6.7	6.6	3.5

In both proportion of subjects having a social increment and in the amount of the increment itself, we thus find that the superiority of the group condition in speed of associations exists throughout the test, but is greatest in the first minute and least in the third minute. The second minute is not far below the level of the first, the drop in increase due to the social influence coming well toward the end of the task.

If we compare the three one-minute periods with each other in the two social conditions separately (Table II.) we find the following. The averages of all subjects alone (20.8, 19.5, and 19.8) indicate a drop in the second minute, followed by a slight rise (probably an end spurt) in the last minute. The averages together, on the other hand, (22.2, 20.8, and 20.5) form a steady decrease to the end. This result suggests that the effect of the group is at first a stimulating and later a steadying one.

Another possible interpretation seems to be that during the first minute when the associations come with great facility, the social influence counts for a relatively greater addition of speed than toward the end of the test when, through fatigue and comparative exhaustion of complexes and vocabulary, the facility of writing associations has de-

creased. Under difficult conditions therefore being alone tends to favor concentration. Group work on the other hand contributes no such benefit to the final and more difficult stages of the task.

(*b*) *Quality of Associations.*—The four general types of association, mentioned under procedure, were counted in each individual test. The average number of each type per test for each subject, in group and solitary work separately, is given in Table V.

TABLE V

AVERAGE NUMBER ASSOCIATIONS OF VARIOUS TYPES

Subject	Alone				Together			
	Personal	Free Rising	Sugg. by Stim. Word	Sugg. by Surroundings	Personal	Free Rising	Sugg. by Stim. Word	Sugg by Surroundings
And.....	8.9	.3	1.9	3.1	2.8	.4	.2	2
App.....	17.8	.4	.1	5.9	24	.4	.4	8.7
Cut.....	20.2	.8	0	0	17.1	2.6	0	0
Hor.....	48.3	.7	.3	1.2	47.7	1.3	1.4	6.8
Hos.....	19.5	.8	1.3	0	13	.6	.5	0
Hun.....	11	1.2	2.7	.3	11.5	1.6	1.5	3.4
Kno.....	1.9	3.4	0	2.4	1.3	2	0	1.7
Lan.....	16.2	.8	.8	2.2	12.8	.5	.7	1.5
Pep.....	19.5	4.5	.4	4.7	15.2	2.8	.3	12.5
Pre.....	7.2	0	1.6	0	7	.1	.9	1.7
Rob.....	45.5	.1	.2	1.5	43.9	.7	.3	4
Spe.....	2.1	0	0	0	2	.1	0	.3
Sto.....	18.5	.1	.1	0	27.3	0	.1	0
Tul.....	15.6	1.1	.2	1.6	12.4	1	0	1.2
Woo.....	11.4	1	.3	1.1	10.1	1.2	1.5	0
Average.	11.4	1.1	.6	1.6	16.5	1.2	.5	2.9
M. V....	8.1	.8	.6	1.3	10.3	.9	.4	2.7

	No. Subjects Having Greater No. Alone	No. Subjects Having Greater No. Together	No. Having Equal No. Alone and Together
Personal Assoc.	12	3	0
Free rising assoc.	5	9	1
Assoc. Sugg. by Stim. Word....	7	4	4
Assoc. Sugg. by Surroundings ..	5	7	3

The first type, and the one yielding the clearest result, is that of *personal* associations. 80 per cent. (12 out of 15) of all the subjects wrote down more personal associations *alone* than together. There is evidently some sort of attitude assumed by the individual in the group which takes him 'out

of himself' and directs his ideas toward outside objects, and as we shall show later, to the actual presence of the others. In the group we are inclined to expand in our thought; we become objective rather than egocentric, present rather than retrospective.

Secondly, *words suggested by the immediate surroundings* appear to be more numerous in the group than in the solitary condition. If we consider the average of all subjects, the tendency is marked, for the average together is almost twice as great as that alone (2.9 to 1.6). Individuals considered, we find that 3 show no tendency either way while 7 produce more words relating to the surroundings in the group, and 5 produce more alone. Hence we find that an environment of active persons is more likely to intrude upon one's trend of thought than an environment of mere space and furniture such as in the solitary condition.

The third type of associations, the 'free-rising' ideas, also occur more frequently in the group. The averages give only a slight increase for the group work; but the individual records show that 64 per cent. (9 out of 14) of persons affected either way produce more of these spontaneous ideas in the group than they do alone. It is possible that 'free-rising' words result here from that greatly facilitated flow of associations characteristic of work in the group.

Finally, we may note that *words*, other than the first, *suggested mainly by the stimulus word* are more numerous in the solitary than in the group condition, as shown by the average of the individuals and especially by the number of individual cases (ratio of 7 to 4). The explanation, though obscure, may lie in the longer persistence of the original trend of thought in the solitary than in the social setting.

(*c*) *Correlations.*—There are, finally, several correlations to be described. The first is that between the rank of individuals in speed of association and their rank in regard to the favorableness of the group influence on their work. We find, in harmony with the results of other investigators, and with the writer's own study of attention and mental work, that there is an inverse correlation—though here it is a very small

one (− .12).[1] Important exceptions act to reduce the index of correlation: for example, Cut is high in both respects; Hor is low.

A further correlation was developed to indicate to what extent the social increments were due to that irreducible minimum of conscious rivalry characteristic of all co-working. It was found that only 3 subjects experienced no rivalry at all. The remainder, 13, all recorded on the average of all tests more rivalry in the group than alone. The ratio of this increase was 1.9 to 1. The correlation between this excess of rivalry consciousness 'together" and the size of the social increment was found to be so slight as to be negligible. It was .23. There was also no correlation (.02) between the subject's report of mere vividness of consciousness of the group (or its absence) and the amount of the stimulation to speed afforded by the group.

(*d*) *Introspection.*—There was substantial evidence from the introspection of awareness of being "*drawn out*" by the presence of the group, so as to produce associations of a more objective type, as previously shown in the results. As to the group influence on speed, two clear cut factors appear in some cases in the same report. The first is an *impeding* influence owing to sensory distraction, emotional factors such as over-stimulation in rivalry, self-prejudicial comparisons with others, and the like. The second and stronger factor is *facilitation.* Numerous stimuli indicative of the rapid work of one's neighbors serve as a drive to greater effort. The principle here involved is probably that commonly known as "suggestion" or "imitation" in superficial accounts of group and crowd phenomena.

Experiment III

Free Chain Association

1. *Procedure.*—The method used in Experiment II. was considered imperfect on the following ground. Associations

[1] All correlations in this paper were obtained by the use of the rank method, using the formula:

$$r = \frac{6\Sigma(d^2)}{n(n^2 - 1)}.$$

are as a rule produced more rapidly than they can be written, and therefore the writing down of each one in turn does not allow the most rapid play of which the process is capable. The results may measure, not the associational ability itself, but simply the speed of writing. A short experiment was accordingly prepared in which the subject was to write down, not *every* word, but every *fourth* word which occurred to him. With a little practice the subjects rendered automatic the rhythm of writing every *fourth* word only. The division of the three minute period into 3 parts was not made in this experiment. The subjects and grouping were the same as for experiment II. The tests were presented on two days, within the period of a week. The sequences of alternation were as follows, the horizontal line dividing the work of the two days:

Groups *A* and *C*		Group *B*	
T			*A*
T			*A*
	A	*T*	
	A	*T*	
T			*A*
	A	*T*	

	A	*T*	
	A	*T*	
T			*A*
T			*A*
	A	*T*	
T			*A*

2. *Treatment of Data.*—The number of associations written in each test was multiplied by 4 and tabulated. The record of Tul is omitted owing to insufficiency of data. The result of the first test was eliminated whenever it was found to be lower than the score of any succeeding test under the same social condition. Table VI. presents the usual summary of individual records.

3. *Discussion of Results.*—The results shown in Table VI. indicate again a distinct though less pronounced advantage for work done in the group. Two subjects show a social equivalence. Of the rest 66 per cent. produce more associations in the group. The group average for the associations together is greater than the average alone, and the average of

the social *increments* is very much greater than the average of the social decrements.

TABLE VI

COMPARISON OF AVERAGE NUMBER OF ASSOCIATIONS. (ALSO PERCENTILE GAINS)

Subject	No. of Trials		Av. No. Associations		Per Cent. of Gain	
	A.	*T*	Alone	Together	Alone	Together
And.	5	6	130	130	—	—
App.	6	5	150	158	—	5.3
Cut.	6	5	166	163	1.8	—
Hor.	6	6	113	117	—	3.5
Hos.	6	6	80	85	—	6.2
Hun.	5	6	82	82	—	—
Kno.	6	6	89	101	—	13.4
Lan.	6	6	117	125	—	6.8
Pep.	6	6	113	111	1.7	—
Pre.	6	6	119	126	—	5.9
Rob.	4	6	121	118	2.4	—
Spe.	5	6	126	129	—	2.4
Sto.	6	6	54	60	—	11.1
Woo.	4	5	120	119	.9	—
Average	5.5	5.7	112.8	116	1.7	6.8
M. V.			21	20.1	.4	2.7

Number of subjects having greater number of associations *Together*.............. 8
Number of subjects having greater number of associations *Alone*................ 4
Number of subjects having equal number *Together* and *Alone*.................. 2

The number of trials was too small to allow the mean variation to carry much significance. Comparing however the mean variation of the social increments with that of the decrements, we find distinctly more variability in the former. In other words the social influence, when it affects workers favorably, affects them also in very varying degrees. Considering, not increments, but actual number of associations written together and alone, the variability both relatively and absolutely is greater alone.

The correlation between the individual's ability to associate rapidly and the size of his social increment is still inverse, and somewhat greater than usual (— .53).

The consciousness of rivalry was in most cases slight in amount, and occurred in slightly over half the individuals. The effect on the work however was perhaps noticeable. Between excess of rivalry together and the size of the social increment there was the small correlation of .41.

Experiment IV

Free Chain Association

1. *Procedure.*—Since the conclusions of experiment III. were based on rather few results the experiment was repeated using fewer subjects but about twice as many tests. This time no stimulus word was given: the subject thought of his own initial word. Another difference was that every *third* word, instead of every *fourth*, was written. The subjects numbered eight and were divided into two groups of 4 each. The experiment covered about five weeks, each group being tested once per week. There was an average of four tests in each hour that the groups were tested. The sequence of social conditions employed a combination of alternations in successive days and alternations within the same day. Group *A* began with *T*, and group *B* with *A*. (In tabulating the number of words set down was the actual number of associations *written*. The individual summaries are presented in Table VII.)

Table VII

Comparison of Average Number of Associations. (Also Percentile Gains)

Subject	No. Trials		Average No. Associations		Percent of Gain	
	A	*T*	Alone	Together	Alone	Together
Aza.	9	9	25.5	27.5		7.8
Cur.	8	10	18.6	19.6		5.3
Han.	5	4	35	37		5.7
Ric.	9	15	24.9	22.8	8.4	
Rot.	8	16	16.9	17.3		2.3
Sli.	9	10	19	20		5
Tan.	8	16	29	29.6		2
Tay.	7	9	47.1	42.8	9.1	
Average	8	11	27	27	8.7	4.7
M. V.			8.2	7.1	.35	1.7

Number of subjects having greater number of associations *Together* 6
Number of subjects having greater number of associations *Alone* 2
Number of subjects having equal number of associations *Together* and *Alone* 0

3. *Discussion of Results.*—The general results of this experiment verify those of the one preceding. A somewhat greater proportion of subjects (75 per cent.) have a social increment. This proportion is still, however, less than the

93 per cent. who attained social increments in the mechanical task of writing down *every* associated word.

The average increment is less than the average decrement owing to the unusually large decrements of the two subjects who did better alone. This fact probably justifies the conclusion of individual differences, the two subjects, Ric and Tay, requiring the solitary condition for their best efforts. Work upon reasoning (see Exp. VI.) corroborates this tendency in the case of Ric.

The mean variation agrees with that of experiment III. in showing a greater variability in work done alone. (The mean variation of increments and decrements is here without value owing to the small number of cases.) We should take into account here the part played by rivalry in producing the social increment of the results. The correlation in this experiment is very high (vide infra). Where rivalry is effective (*i.e.*, not merely present) in group work there is a tendency toward uniformity among the members.

The correlation between the subject's associative ability and the degree to which his work is increased by the group is still inverse, though small (– .19). Here also we find toward the extremes conspicuous exceptions to the inverse correlation.

We noted in our last experiment a slight tendency for conscious rivalry to become a cause, or at least an accompaniment, of the group stimulus. We now find that tendency verified. There was found a positive correlation of .89 between the excess of consciousness of rivalry together and the size of the social increment. Rivalry therefore plays a greater part in the speed of associations for the most part merely *thought* than it does in the case of associations whose flow is limited to the speed of their *writing*. This result is doubtless due in part to the readier improvability of the mental association speed than of the speed of writing.

The significance of inhibitions in producing the solitary decrement is suggested by a positive correlation between an excess of inhibitions alone and the tendency to improve in the group. The correlation found (.76) indicates that the low

level of the subject's work alone is connected with his inhibitions—these inhibitions occurring for him more numerously in the solitary than in the group condition. Alone there is evidently a blocking, traceable perhaps to a lapse of attention or effort, or to forgetting "where one is." Under the stimulus of the group the response receives greater facilitation.

Experiment V

Controlled Association—Contrasted Occupations

As a minor digression from the usual setting of the experiments, an attempt was made to compare not the work of the subject alone with his work in the group, but to compare his accomplishment, always in the group, in tasks *similar* with that in tasks *opposite* to the occupations of his fellows. Half of the group were seated at one side of the table and instructed to write words all bearing upon one specified topic (*e.g.*, winter), while the other half, seated opposite, wrote words all bearing on the opposite theme (summer). The solitary condition was not used in this experiment.

A test pair consisted of two performances upon a given theme, one the result of working in the manner described above, the other produced at another time when all members of the group wrote upon the *same* topic. Only about three such test pairs were obtained from each subject. Their total for the experiment was 35. Of these pairs 19 showed an excess of associations written in the common occupation over those written in contrasted work. In only 16 pairs was the advantage with the contrasted occupation. The average excess also in the common work was greater (7) than the average excess where it occurred in the contrasted setting (5).

So far as these results go there seems to be an advantage in tasks in which all are working in agreement over work done while pursuing diverse trends of thought. The tests given are however too meager for certainty; they indicate merely an interesting possibility for further investigation.

Experiment VI

Thought Process

1. *Procedure.*—An experiment was finally performed which extended the study of the social influence to the more intellectual functions involved in reasoning. Statements have been made by various writers that this "higher" quality of process is better performed in solitude. Tests of critical and original thinking were therefore devised in the nature of discursive reasoning. Short passages were selected from the works of Epictetus and Marcus Aurelius which admitted of considerable argument, for and against. The task of the subjects was to write down all the arguments, as many and as strong as possible, which they could think of to *disprove* the point made in the passage given. The epigrams both together and alone were presented in legible handwritten form, one copy to each subject. At the beginning of the group tests it was emphasized that they were all writing on the same statement. The time allowed for writing the ideas in a single test was 5 minutes. A separate passage was used for each test.

Nine subjects were used, arranged in two groups, *A* and *B*. Approximately 20 tests were given alone and 20 in the group. The total period covered by the experiment was 2 months. The social condition was changed (from *A* to *T*, or from *T* to *A*) on successive days. Group *A* began with *T*, group *B* with *A*. The passages selected naturally varied somewhat in suggestiveness; but it is believed that in a series of 20 a fair uniformity was obtained for the two social conditions. This is still more likely since only two authors were used, and those two are singularly constant in the tenor of their utterances.

2. *Treatment of Data.*—Each test written was graded as to quality of the arguments proving the negative of the statement. For this purpose the following scale was used. A distinct, clear, and (for the subject) forceful idea going directly to the question received a score of 3. Developments, extensive illustrations of the point made, giving pertinent

opinion of an authority, refining or re-stating the question to remove its logical objections, suggesting an alternative proposition, and the like, each counted 2. Emphatic statements or interjections of rejection, quotations merely stating the opposite, personal aphorisms, repetition of an argument already given, qualification or withdrawal or a former argument, rather irrelevant arguments, and so on, were each scored 1. For each subject each type of idea or argument thus contributed its proper score to each test; and by averaging the sums of these scores for the various tests there were found the average individual scores for ideas. These results are presented in Table VIII.

TABLE VIII

REASONING

Average Scores for Ideas

Subject	No. Trials		Score of Ideas		Per Cent. of Gain	
	A.	*T.*	Alone	Together	Alone	Together
Aza	19	22	5.6	6.6		18
Cur	19	16	7.5	7.6		1.3
Han	15	24	12.6	14		11
Ric	23	19	9.7	9.7		
Rot	17	20	4.5	4.8		6.6
Sli	19	20	9	10.2		13.3
Tan	23	19	7.7	8.2		6.5
Tay	2	5	9	10.2		13.3
Wil	12	15	7	8.4		20
Average	16.6	17.7	8	8.8		11.2
M. V.			1.7	1.9		4.9

Number of subjects having higher idea score *together* 8
Number of subjects having higher idea score *alone* 0
Number of subjects having equal idea scores *together* and *alone* 1

In addition, the total number of ideas of the three types, together and alone, found in the work of each subject, were averaged separately. Table IX. presents this comparison of averages of ideas types. Taking the number of each type as a per cent. of the total number of ideas in the given social condition (*i.e.*, together or alone), one may compare the relative contributions, together and alone, of the several types to the total idea score for the subject. This comparison is shown in Table X. Finally the words written in each test

were counted, and the average number of words per test for the different individuals, together and alone, was computed. Table XI. contains these averages together with the percentile gains under the proper social condition.

Table IX

Average Numbers of Idea Types

Subject	Average Numbers per Test of the Various Types; 3's, 2's and 1's[1]					
	Alone			Together		
	3's	2's	1's	3's	2's	1's
Aza	1.3	.6	.3	1.5	.9	.2
Cur	1.5	1.3	.3	1.6	1	.6
Han	3.2	1.2	.3	3.5	1.4	.6
Ric	2.3	1.1	.3	2.2	1.3	.4
Rot	1	.5	.6	1.2	.4	.4
Sli	1.9	1.5	.1	2.5	1.2	.3
Tan	2.4	.1	.0	2.6	.1	.05
Tay	2.5	.5	.5	2	2	.2
Wil	1.7	.7	.4	1.8	1.2	.6
Average	1.97	.8	.31	2.1	1.0	.36

Table X

Percentile Comparison of Idea Types

Subject	Per Cent. of Average Total Ideas Comprised by Ideas of the Various Types—3's, 2's, 1's					
	Alone			Together		
	3's	2's	1's	3's	2's	1's
Aza	59	27	14	58	34	8
Cur	48	42	10	50	31	19
Han	68	26	6	64	25	11
Ric	62	30	8	57	33	10
Rot	48	24	28	60	20	20
Sli	54	43	3	63	30	7
Tan	96	4	0	94	4	2
Tay	72	14	14	48	48	4
Wil	61	25	14	50	33	17
Average	63	26	11	60	29	11

[1] These figures are *numbers* of cases only. The idea scores, as given in Table VIII., were obtained by multiplying the number of 3's by 3, the 2's by 2, and the 1's by 1. and then adding these products. This was done in the separate tests however, since the averages given in Table IX. contain small inaccuracies due to decimals.

Comparison of Percentages

Type 3	Number of subjects having higher percentage for type 3 together	3
	Number of subjects having higher percentage for type 3 alone	6
Type 2	Number of subjects having higher percentage for type 2 together	4
	Number of subjects having higher percentage for type 2 alone. (1 subj. equal)	4
Type 1	Number of subjects having higher percentage for type 1 together	6
	Number of subjects having higher percentage for type 1 alone	3

Table XI

Average Scores for Words Written

Subject	Score of Words		Per Cent. of Gain	
	Alone	Together	Alone	Together
Aza	69	90.4		31
Cur	81.4	80.9	.6	
Han	148	144.1	2.6	
Ric	98.4	108		9.7
Rot	29.8	39.6		33
Sli	119.7	125.6		4.9
Tan	58.6	67.5		15.1
Tay	158	167.8		6.2
Wil	100.4	96.2	4.1	
Average	96	102	2.6	16.6
M. V.	32.1	30.3	1.1	10.2

Number of subjects having higher word scores together........................ 6
Number of subjects having higher word scores alone........................... 3
Number of subjects having equal word scores together and alone................ 0

3. *Discussion of Results.*—From Table VIII. it will be seen that of the 9 subjects used, 1 had equal average idea scores together and alone. The remaining 8 *all* had higher idea scores when working in the group. The average idea score together for all subjects showed also a social increment: it was 8.8, while the average alone was 8. The individual social increment, generally rather large, ranged from 1.3 per cent. to 20 per cent., 5 out of 9 being above 10 per cent.

Turning to the percentile importance of each idea type alone as compared with its importance together, as shown in Table X., we find that 6 out of 9 subjects had a higher percentage of superior ideas (counting 3) while working alone. The number of the subjects, together and alone, having the higher percentage of "2" ideas was *equal.* There were (reciprocally to the "3" class) just 6 out of 9 subjects who

had a higher percentage of the lowest type of ideas (counting 1) while working in the group. The averages of all subjects indicate also a higher performance, relatively, so far at least as the proportion of superior ideas is concerned, while working alone. There is thus demonstrated a social subvaluent for argumentative or discursive reasoning. This finding is no doubt in accord with commonly observed facts of life. Who has not been aware, upon retrospection, of the low order of logical value in many arguments given under such a strong social influence as that of political meetings and oral debates? There seems to be a *spreading out* of our throught rather than a strong output of separate original ideas of logical worth. Group thought is *extensive*; individual thought is, to some extent, *intensive*.

May not this "extension" in group thought be also characterized as "wordiness"? It seems quite logical to call it this, for Table XI. shows that 6 out of 9 subjects wrote more words in the group than they did alone. The averages of the individuals' scores also show the group gain in number of words written (102 to 96). A third evidence lies in the excess in the average of the social increments (16.6) over the decrements (2.6). This disclosure is consistent with the results of all previous experimentation on the social influence. There has been throughout a clear increase of the *quantitative* aspect of mental processes and mental work in the group condition. Our association experiment of writing every word in free thought may be compared with the writing of every word in controlled thought in the present experiment. In both cases every subject but one showed, in the quantity of words written, a distinct social increment.

4. *Introspection.*—There is some evidence in the reports of the awareness of the social subvaluent in the thought process.

SUMMARY OF CONCLUSIONS

A. The Influence of the Group upon Association

I. *Quantitative Aspects*

1. The main result of the preceding experiments on association is the conclusion that the *presence of a co-working*

group is distinctly favorable to the speed of the process of free association. In various tests from 66 per cent. to 93 per cent. of the subjects show this beneficial influence of the group.

2. The beneficial group influence is *subject to variation according to the nature of the task.* In the more mechanical and motor requirements, such as writing *each word* associated, the group stimulus is more effective than in the more highly mental or more purely associational tasks such as writing only every *third* or *fourth* word.

3. There are *individual differences* in susceptibility to the influence of the group upon association. One type, who are nervous and excitable, may succumb to the distracting elements of the group activity and may show either no effect, or else a social decrement.

4. *In its temporal distribution* the beneficial effect of the group is greatest in the first part of the task and least toward the end of the task.

5. There is a tendency for the *slow individuals to be more favorably affected* in speed by the group co-activity than the more rapid workers. There are, however, certain striking exceptions.

6. *The variability in output* among the individuals varies generally with the social influence. Hence it is usually greatest in the group work. A striking exception to this occurs in the tests where rivalry is correlated with the social increment, and where only every third or fourth word is written. Here the variability is greatest in the solitary work. This result is in agreement with that of earlier investigators working on different processes.

7. There is suggestive but *not conclusive* evidence that the output of associations in a group where all the members are forming associations in the same category is greater than that in groups in which the members are divided in the trend of their associations between opposite or contrasted categories.

II. *Qualitative Aspects*

8. A greater number of *personal associations* are *produced alone* than in the group.

9. In harmony with this fact is the tendency for subjects to produce *ideas suggested by their immediate surroundings* with *greater frequency in the group* than alone.

10. Less clear cut, but very probable, are the tendencies to produce a *greater number of "free rising" ideas in the group*, and to produce a greater number of words *suggested mainly by the initial stimulus word when working alone.*

III. *Factors in the Social Influence*

11. There are two opposing groups of factors in the influence of the social condition upon the association process. They are:

(1) Facilitating Factors:

(*a*) *Facilitation of movement* by perceptions or ideas of movements in others near us.

(*b*) *Rivalry* intrinsic in the bare social setting of a group working together. Rivalry is well correlated with the beneficial influence of the group in tests of a more mental sort (and less mechanical) such as writing every *fourth* word only. It is not so correlated when each word is written.

The beneficial effects of the group in experiments where the rivalry consciousness is closely correlated with this influence is less than in experiments where it is not so correlated, but where other factors—for example, motor facilitation—serve as the stimulus of the group.

(2) Impeding Factors: distraction, over-rivalry, emotions. Of the two groups, the facilitating is by far the more important in the total effect upon the work.

12. Beside the comparisons already indicated, we may note the general agreement of our work with that of earlier students in the *speed* improvement of mental operations, as shown by the quantity of the product, under conditions of working with others.

B. The Influence of the Group upon the Thought Process

13. In the highly controlled association of the thought process, as typified in written argument, more ideas are pro-

duced in the group than when working alone. Again we find an increased flow of thought owing to the social stimulus.

14. Among the ideas so produced, those of superior quality, however, are of relatively greater frequency in the solitary than in the group work. Ideas of a lower logical value are relatively more numerous in the group work.

15. More words are used in the arguments produced in the group than in those produced in solitude.

16. From the above facts, and also from the introspection of the subjects, we may conclude that the presence of the group influences the reasoner toward a more conversational and expansive form of expression. The more intense logical thinking of solitude gives way in the group to extensity of treatment.

17. These results appear to be related to the common observation that work requiring imagination or more concentrated and original thought is best performed in seclusion. There is also a connection suggested with the writer's experiments upon the social influence in attention and mental work.[1] In that investigation, as well as in the present, the social influence was found to improve the quantity but not the quality of the mental performance.

[1] To be published in the near future.

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THE EFFECT OF UNIFORM AND NON-UNIFORM ILLUMINATION UPON ATTENTION AND REACTION-TIMES, WITH ESPECIAL REFERENCE TO STREET ILLUMINATION

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A. INTRODUCTION

The problems of street illumination have not been approached hitherto from a psychological standpoint. The principal considerations have been photometric and æsthetic, but it is evident that these are insufficient. Mere ability to discern surface irregularities, vehicles, pedestrians or obstacles on the street or sidewalk does not insure safety unless the reaction to these stimuli is normal. If, under a given system of illumination, the reactions of the driver or pedestrian are retarded and the degree of attention diminished, the danger is manifest. From casual observation it would appear that some such factor is present. Many automobilists speak of feeling "sleepier" under certain varieties of street-lighting. Moreover on theoretical grounds the monotony of some lighting systems might be expected to influence the mental state.

During the summer of 1914 the writer was connected with an experimental study of street illumination conducted by the Street Lighting Committee of the National Electric Light Association on Intervale Avenue, New York City. It was found feasible to test the attention and reaction-times of a subject walking on the street under various conditions of illumination. The results thus obtained proved rather significant and the following winter the problem was reduced to laboratory form and experiments conducted in the Harvard Psychological Laboratory. The following article thus comprises two parts, an account of the experiments performed

upon the street and of the supplementary experiments in the laboratory.[1]

B. Experiments on the Street

I. *Equipment*

The following experiments were conducted at Intervale Avenue, Bronx, N. Y. Two sections of the avenue, each a third of a mile in length were equipped for experimental purposes. In one section series circuits fed lamps mounted at the curb 14 ft. above the street, staggered at 50 ft. intervals, with an alternative system of centrally hung lamps 24 ft. above the street at 150 ft. intervals. In the other section the lamps were suspended from span wires and could be moved to any position transverse the street, or from a height of 18 to 30 ft. Provisions were made for mounting series lamps at 75 ft. intervals with two series circuits feeding alternate lamps. The circuits were operated from tub transformers in a temporary sub-station supplied by a 2,200 volt circuit from the Hunt's Point sub-station of the New York Edison Company. The street was 60 ft. wide from curb to curb and paved with Belgium block. Dwelling houses, small apartments and a few stores lined the sides. The street was reasonably free of traffic during the evening.

II. *Method.*

Certain members of the Street Lighting Committee conducted experiments designed to bring out the ability to discern surface irregularities and obstacles in the street under various conditions of illumination. The writer confined himself to attention, reaction times and motor coördination. The efficiency of the eye, while undoubtedly of importance where a person is exposed to street illumination for a prolonged period, as in the case of the motorman, was not studied. It was deemed advisable to restrict the experiments to relatively short periods of continuous exposure to a given illuminating system—the condition encountered in the case of the average pedestrian.

[1] The writer expresses his obligations to Professors Hugo Münsterberg and Herbert S. Langfeld and to Messrs. Preston S. Miller and W. F. Little, of the Electrical Testing Laboratories, New York City.

To measure the quickness of response to unexpected stimuli and the ability to make a quick association and carry out the appropriate movement, the auditory choice reaction was adopted. Auditory rather than visual stimuli were used because they left the subject free to look down the street as he would normally. It was desired to reproduce as closely as possible the situation that is met in practical conditions, and with red and green lamps for stimuli the subject would tend to keep his eyes fixed on the point from which the stimulus was to come rather than directed down the street. Two electric bells of distinctly different pitch were connected in parallel. In series with them were two presselles at the end of 10 ft. of lamp cord, a signal magnet and dry cells. The magnet and a 100vd. electric tuning fork recorded on a kymograph. The whole apparatus was mounted rigidly and clamped to a table built on the truck of a baby carriage. The subject walked down the traffic line midway between the center of the street and the curb, holding a presselle in each hand. The experimenter followed at a distance of about 8 ft. wheeling the apparatus. The subject was instructed to look down the street as he would under ordinary conditions, and when he heard the "cow-bell" to release the right hand and when he heard the "door-bell" to release the left. Trials for the right and left hands were given in irregular orders and at irregular intervals without warning. Right and left reactions registered similarly and were averaged together, approximately the same number of both being given in a series. Incorrect reactions were few (not more than 5 per cent.). The subject reported mistakes and such reactions were omitted from the results. The subject walked continually except when it was necessary to stop a moment to shift the drum of the kymograph. The subject always passed through the complete distance between lamps or multiples thereof, depending on the spacing, so that reactions were made at approximately all degrees of illumination under a given system. Fifteen to twenty reactions constituted a series according to the capacity of the drum, *i. e.*, from 60 to 80 reactions for a subject on a given comparison.

A form of short exposure test was adopted as a measure of attention. A box was constructed 30 × 30 × 30 cm. with a 15 cm. tapering extension on the top through which the subject could look at a field 45 cm. from the eyes. The interior was illuminated by two battery lamps screened from the eyes of the subject and controlled by a switch and an opening at the bottom of one side of the box allowed cardboard material to be inserted. This tachistoscope was clamped on the baby-carriage. Simple geometrical figures such as a square, circle, trapezoid, etc., constituted the material. It was desired to test not merely memory for a series of separate figures, but rather the ability to analyze them out of a complex group, which necessitated a high degree of attention. Accordingly five were drawn in outline with ink on light gray cardboard 30 × 30 cm. so that four of them touched at the center of the card, above, below, right and left, and the fifth was concentric with the field. The subject placed his eyes at the opening and one second after the signal "ready" the field was exposed for one second by closing and opening the switch in time with the swings of a half-second pendulum attached to the side of the tachistoscope. Immediately after the exposure the subject drew upon a pad as many as possible of the figures in the correct position.

Exposures were made at intervals of 15 ft. along the traffic line, a new permutation of objects being shown each trial. When walking between trials the subject kept his eyes directed down the street as he would normally. In scoring the results a figure in the correct position was counted 2, and a figure merely disoriented or displaced less than a quadrant from the correct position counted 1. The benumbing or quickening effect of a given illuminating system on the mental setting would presumably not disappear in the few seconds that elapsed while the subject was lowering his head to the opening of the tachistoscope and awaiting the exposure after the signal "ready." The gray cardboard was used as it afforded less contrast with the street surface upon which the subject had been gazing a moment before. Ten exposures usually constituted a series, in some cases twenty—*i. e.*, forty or eighty

trials on a given comparison. In some instances several subjects (never more than five) were used at the same time, each one stepping aside to make his drawing while the next one observed the figures. Care was taken that no exposure should be made while there was any distracting motion among the subjects.

As a measure of motor coördination a "three-hole" test was employed. A board 20 × 20 × 1 cm. had three 12 mm. holes bored in it at the vertices of an equilateral triangle 10 cm. on a side. A brass plate covered the bottom of the board and was connected in series with a signal magnet and dry cells. A tapping stylus formed the other pole of the circuit and contacts made at the bottom of the holes were registered by the signal magnet on the kymograph. The subject was instructed to insert the stylus in each hole successively as rapidly as possible, being sure to touch the brass plate at the bottom of each hole before passing to the next. The results were thus reduced to terms of speed. The test was always performed under a lamp where the holes could be plainly seen, the inside of them being painted black to render them more visible against the white board. The actual illumination of the board was not of importance as long as the holes were clearly visible and there was no distracting reflection of light from the board. The subject tapped for 30 seconds from the signal "ready—go" until the signal "stop." Four or five trials were given after each choice reaction series. Between trials the subject looked down the street.

The main interest of the experiment was in the comparative results under any two systems of lighting. With each subject two equipments were used on the same evening, so that variables such as temperature, weather and physiological condition were fairly constant. The tests were always performed in both time orders—equipment *A* followed by equipment *B* and then after an interval of 10 or 15 minutes, *B* followed by *A*. The average of the two series on the first equipment was compared with that of the two series on the second. A short practice series was given each subject before proceeding

to the street for the first series on a given method. The subject always walked for 5 minutes on a given street before being tested.

For all the above experiments only short sections of the two streets were used. This was because one street passed through a square with cross traffic and a number of brightly lighted stores. Two short sections were selected which were fairly similar in conditions along the side. In one case there was a small garage and in the other a delicatessen store on the side of the street used and these were frequently closed during the later hours. Precautions were taken against undue distraction in all the experiments. No trials were made when a vehicle of any sort was passing or when there was any unusual disturbance on the sidewalk. Conversation during a series was avoided. In only two or three cases were the subjects at all aware of the trend of previous results. All possibility of suggestion was avoided. The subjects were employes of the Electrical Testing Laboratories of New York City, and of the New York Edison Company, the majority of them between twenty and thirty years of age. The experiments were performed between 8 P.M. and midnight.

III. *General Results*

Experiments carried on by the above methods indicate that of the illuminating factors influencing the attention and mental reactions, uniformity and non-uniformity of illumination are paramount. The main thesis that seems established by the experiments on the street is that under a distinctly non-uniform illumination (with a mean variation in foot-candles of upwards of 50 per cent.), the attention of an individual walking through the street during a period of 20 to 30 minutes is at a higher level and his reaction time is quicker than when he is walking under a distinctly uniform illumination during a similar period.

Table I gives the results of all the experiments in which a markedly uniform system of lighting was compared with a markedly non-uniform. In the first column are given the two equipments compared on a given evening. Nitrogen gas

filled Mazda lamps were always used. To produce uniform illumination, 80 candle power lamps in Holophane units with refractors were mounted 14 ft. above the curb, staggered at 50 ft. spacing, or 250 c.p. lamps in Holophane units with refractors were mounted 24 ft. above the center of the street at 150 ft. spacing. The non-uniform illumination was usually produced by 250 c.p. lamps in diffusing globes (the Edison AB unit) centrally hung 24 ft. above the street at 150 ft. spacing, or by 400 c.p. lamps in Edison units mounted 30 ft. above the street 7½ ft. out from the curb and staggered at 300 ft. intervals. In a few cases bare lamps were used. In the first column of the table the first figure indicates the mounting and the second the spacing followed by the type of unit and the candle power. The next two columns indicate roughly the difference in uniformity of the two equipments under comparison. Measurements of horizontal and vertical illumination were made with portable photometers at thirty stations on a typical block of the street between two lamps for each of the equipments used. The mean variation of these thirty measurements expressed as a per cent. of the mean illumination represents roughly the degree of uniformity of a given system. In the table the per cent. mean variation of the non-uniform street is divided by that of the uniform that was compared with it for both horizontal and vertical illumination. The larger figure in the table thus indicates a greater difference in uniformity between the two equipments compared. The remaining colums give the results of the individual series on each of the three methods. The names of the subjects are not given because, with a few exceptions, the same subject did not participate on more than one evening and consequently stress is to be laid not on individual differences but on general tendencies. The three columns under each group give respectively the per cent. of superiority of the average results of a subject under the non-uniform equipment, the difference between his averages on the two equipments divided by the probable error of difference, and the mean variation (in per cent.) of the results under the uniform lighting divided by that under the non-uniform. The mean variation

Table I

Equipment				Difference in Uniformity		Choice Reaction			Tachistoscope			Motor Coördination		
Mount	Space	Type of Unit	C.P.	Hori-zontal	Vertical	% Superiority Non-uniform	Difference / P.E.	M.V. Uniform / M.V. Non-uniform	% Superiority Non-uniform	Difference / P.E.	M.V. Uniform / M.V. Non-uniform	% Superiority Non-uniform	Difference / P.E.	M.V. Uniform / M.V. Non-uniform
24	150	Edison	250	2.32	1.25	30.0	5.5	1.07	13.6	2.2	1.52	12.1	4.6	1.49
14	50	Holophane	80	...	...	28.4	7.4	.88	− .7	.2	1.86	5.3	1.6	3.46
..	..	....	..	...	...	9.2	2.8	.86	5.1	.7	1.18	20.0	7.7	2.02
..	..	....	..	...	...	6.4	3.2	.75	28.0	3.4	2.20	5.4	1.7	1.62
..	..	....	..	...	...	9.7	2.6	.76	5.7	1.0	1.51	5.5	1.1	.40
..	..	....	..	...	...	....	...	...	....	...	...	7.1	1.4	1.07
..	..	....	..	...	...	....	...	...	....	...	...	− .7	.1	1.10
..	..	....	..	...	...	....	...	...	....	...	...	− 8.1	1.0	.56
..	..	....	..	...	...	....	...	...	....	...	...	10.1	1.7	1.09
..	..	....	..	...	...	....	...	...	....	...	...	− 5.6	1.1	1.57
30	300	Edison	400	3.06	2.09	46.2	6.4	1.23	13.1	1.9	1.13	2.0	1.3	1.14
14	50	Holophane	80	...	...	11.7	3.5	1.79	9.6	1.8	.74	− .4	.3	1.98
..	..	....	..	...	...	16.8	2.5	.69	3.9	.6	1.06	4.4	1.5	2.08
..	..	....	..	...	...	10.7	3.1	1.25	− 5.8	.8	1.15	....	...	...
..	..	....	..	...	...	....	...	...	14.1	3.9	1.01	....	...	...
..	..	....	..	...	...	....	...	...	− 3.8	.5	.82	....	...	...
..	..	....	..	...	...	....	...	...	5.7	.9	1.08	....	...	...
..	..	....	..	...	...	....	...	...	1.0	.2	2.02	....	...	...
..	..	....	..	...	...	....	...	...	11.0	1.5	.94	....	...	...
..	..	....	..	...	...	....	...	...	16.8	9.9	2.00	....	...	...
..	..	....	..	...	...	....	...	...	10.3	2.9	1.70	....	...	...
..	..	....	..	...	...	....	...	...	2.0	.2	1.90	....	...	...
..	..	....	..	...	...	....	...	...	14.9	1.9	1.20	....	...	...
..	..	....	..	...	...	....	...	...	16.8	2.0	2.04	....	...	...
..	..	....	..	...	...	....	...	...	− 2.7	.5	1.27	....	...	...

Equipment				Difference in Uniformity		Choice Reaction			Tachistoscope			Motor Coördination		
Mount	Space	Type of Unit	C.P.	Horizontal	Vertical	% Superiority Non-uniform	Difference / P.E.	M.V. Uniform / M.V. Non-uniform	% Superiority Non-uniform	Difference / P.E.	M.V. Uniform / M.V. Non-uniform	% Superiority Non-uniform	Difference / P.E.	M.V. Uniform / M.V. Non-uniform
24	150	Edison	250	1.32	1.25	15.1	2.8	.76	− 8.6	1.5	1.26	5.5	1.6	1.42
24	150	Holophane	250	...	...	8.5	2.3	1.05	7.2	1.1	.78	3.4	2.4	1.03
..	..	....	..	...	...	15.4	3.9	1.03	1.8	.3	1.11	5.0	3.1	1.05
..	..	....	..	...	...	13.2	3.5	1.01	13.3	2.3	1.54	1.0	.9	1.00
..	..	....	..	...	...	15.1	5.3	1.25	9.2	1.7	.90	6.3	1.1	.68
..	..	....	..	...	...	14.1	3.4	1.27	20.2	3.2	1.22	1.3	.6	2.16
..	..	....	..	...	...	....	...	...	17.5	2.1	1.73	....	...	...
..	..	....	..	...	...	....	...	...	6.8	1.1	1.34	....	...	...
30	300	Edison	400	1.74	2.10	....	...	...	....	...	...	4.0	1.9	.89
24	150	Holophane	250	...	...	....	...	...	....	...	...	1.2	.4	2.63
..	..	....	..	...	...	....	...	...	....	...	...	− 5.3	2.2	2.08
24	150	Edison	400	1.96	1.34	32.3	6.5	.71	5.1	7.7	.73	....	...	...
14	50	Holophane	80	...	...	....	...	...	....	...	...	....	...	...
24	150	Edison	250	1.23	.90	10.8	2.3	.64	....	...	...	....	...	...
14	50	Bare	80	...	...	....	...	...	....	...	...	....	...	...
24	150	Bare	250	1.18	.96	18.2	3.6	.66	....	...	...	....	...	...
14	50	Bare	80	...	...	....	...	...	....	...	...	....	...	...
24	300	Holophane	250	2.16	2.08	17.0	3.9	.98	12.5	2.3	1.34	....	...	...
14	50	Holophane	80	...	...	...	...	...	7.0	.8	1.01	....	...	...
Average	..	....	..	...	...	17.0	3.9	.98	8.1	2.0	1.32	3.6	1.8	1.48

of almost any series of tasks performed under properly controlled conditions is regarded by many as an index of attenttion, the higher mean variation indicating the lower degree of attention.[1] Thus a larger figure in the table might indicate a higher degree of attention under the non-uniform system.

The table shows that in every case the auditory choice reaction time is shorter under the non-uniform equipment and on the average is 17 per cent. shorter. The differences are all greater than twice the probable error of difference and average nearly four times as great. The differences between the per cent. mean variation under uniform and non-uniform lighting is small. Attention, as indicated by a tachistoscopic test, is superior under non-uniform illumination in 26 out of the 31 series. The average superiority, including the negative values, is 8 per cent. The differences are on the average twice the probable error of difference. The mean variation is considerably greater, about 30 per cent. on the average, under the uniform condition. In motor coördination three fourths of the series show greater efficiency under non-uniform lighting, with an average superiority, including negative values, of 3.6 per cent. The differences are on the average nearly twice the probable error of difference. In most cases the mean variation is greater under the uniform conditions—48 per cent. on the average.

The above results are more briefly indicated in Table II. The successive columns give the number of series performed under each of the three methods, the per cent. of that number in which the non-uniform series is superior, the average superiority of the non-uniform, the per cent. of series in which the greater mean variation (*i. e.*, lower degree of attention) is shown under the uniform lighting and the average of the quotients obtained by dividing the mean variation of the uniform by that of the non-uniform. The general tendency in favor of a quicker reaction and coördination and a higher degree of attention under the non-uniform equipments is evident.[2]

[1] Compare Pillsbury, "Attention," p. 89ff.

[2] Simple auditory reactions were tried with two subjects and yielded results similar to those with choice reaction. These two types were correlated later in the laboratory work.

TABLE II

	No. of Series	% of Series in Which Non-uniform Superior	Average Superiority Non-uniform, %	% of Series in Which Uniform Had Greater M.V.	Av. M.V. Uniform / M.V. Non-uniform
Choice reaction	18	100	17.0	50	.98
Tachistoscope	31	84	8.1	81	1.32
Motor coördination	22	77	3.6	82	1.48

IV. *Checks*

A number of other variables that might have been instrumental in producing the above results were ruled out by suitable check experiments.

As a check on the possible influence of the nature of the street in distracting elements tests were made on a few subjects with identical equipments on the two streets. The average differences were 6 per cent. and 2 per cent. on reactions and on the tachistoscope respectively, insignificant in comparison with the averages of Table I. Moreover the uniform equipment was sometimes on the north street and sometimes on the south street with little difference in results. Further, the same street was occasionally employed for all four series on a given evening. Here the sidewalk, traffic and window lighting conditions were constant, the only variable being the illuminating conditions. The average superiority of the non-uniform equipments under such conditions differs only slightly from the averages of Table I.

As to the type of illuminant used and the possible effect of Edision *vs.* Holophane units, some reaction series were made, using bare lamps in one or both streets. A street equipped with Edison units, compared with one identical but minus the globes yields slight differences in reaction. But if the Edison equipment is compared with bare lamps with refractors giving uniform illumination, or if two systems of bare lamps are used, one with refractors, the usual differences occur.

As to the intensity of illumination, the photometric data shows that in approximately half of the comparisons the mean intensity of the non-uniform system was greater than that

of the uniform and in the other half the reverse was true. The experimental results favored the non-uniform lighting under both conditions.

The time error was checked as above indicated by working on both equipments on a given evening in both time orders. Further, the uniform equipment was tested on the first series in about half of the comparisons.

As to the possible influence of number of lamps, cross street location, height or spacing, 250 c.p. lamps in Edison units were centrally hung 24 ft. high at 150 ft. spacing and compared with similar lamps in Holophane units with refractors having exactly the same mounting. The results are included in Table I. in the third horizontal group—6 series in choice reaction and motor coördination and 8 on the tachistoscope. In a few of these series moreover the two equipments were on the same street. In this rather crucial check it is evident that the usual tendency is manifest to about the usual degree.

Finally the question might be raised as to whether the superior results under non-uniform illumination are not due to a sort of voluntary alertness produced by the difficulty in seeing objects in the dark areas, that is, whether the results are not due to the dimness of the lighting rather than the non-uniformity. As a partial answer one subject was tested in reaction times on a uniformly lighted street run at the usual 7.5 amperes and then at 6.1 amperes. The reactions were 16 per cent. quicker under the former condition. This would indicate that the dimness factor was not responsible for the results of Table I.

V. *Degrees of Non-uniformity*

The above results point rather strongly to the superiority of non-uniform illumination of the types studied in its effect upon attention and reactions of the pedestrian. Whether the degree of attention varies with the degree of non-uniformity is doubtful. In the present study there were two non-uniform equipments that were used for the most part, with

150 ft. and 300 ft. spacing, the latter of which was more non-uniform in both horizontal and vertical illumination. Both of these equipments were frequently compared with a uniform system which might be regarded as the standard. The 300 ft. spacing shows a greater average superiority over the uniform system in choice reaction than does the 150 ft. spacing, but in motor coördination and attention this condition is reversed. A few tests were made comparing directly the two non-uniform equipments on the same evening with the same subjects. The 300 ft. system was slightly superior by all three methods.

This problem of degrees of uniformity could be answered only by exhaustive investigation. Even if the degree of attention were found to be a positive function of the degree of non-uniformity, it is probable that beyond a certain point the dimly lighted stretches of street would be so broad that the effect would amount to uniformity and produce retardation.[1]

C. Experiments in the Laboratory.

In transferring the problem to the laboratory there were five questions in mind:

1. How far do results obtained with the factor of uniformity and non-uniformity of illumination controlled in the laboratory substantiate in general the results obtained on the street?

[1] The results of the experiments carried on at the same time by other investigators to determine the revealing qualities of the different equipments for surface irregularities and obstacles in the street, tend somewhat in the same direction as the above results, rather than in the opposite direction. Targets of wood 2 inches thick or of galvanized iron plate the size of the surface of a paving block, painted gray and sanded were planted systematically through the two streets. Subjects walked along the traffic line at their normal rate, recording on a pad all the targets they saw and their location. It was found in general that these targets were perceived somewhat more readily on the street with the large illuminants widely spaced than on that uniformly lighted by small illuminants closer together. Furthermore with the equipments studied the targets were found almost as successfully in the darker areas between lamps as in the lighter areas. In other tests sections of stove-pipe one foot high, painted gray and sanded, were planted at various positions and the subjects in an automobile rode through the streets at constant rate with a device which recorded the maximum distance at which each target was seen. Results thus obtained tend in general in the same direction as those with the surface targets. The above results are not as yet published and no conclusions should be drawn from them, but their general tendency is of interest in the present connection.

2. How far do other kinds of reaction correlate with auditory reactions under the various illuminating conditions?

3. What are the effects of different degrees of non-uniformity?

4. Can the results in favor of non-uniform illumination on the street be attributed to greater voluntary effort in watching for obstacles in the dark regions between lamps, *i. e.*, does voluntary attention to one stimulus quicken reaction to another?

5. Can the results be attributed predominantly to the changing illumination through which the subject walks or to the light and dark patches on the street surface?

I. *Equipment*

The experiments were performed in an interior room with black walls, indirectly illuminated by light reflected from the white ceiling. The source was about 1.5 meters from the center of one end of the room directly above the head of the subject, and consisted of a 100-watt tungsten lamp in a box 60 cm. square and 45 cm. deep suspended 60 cm. from the ceiling. The subject sat alone, the experimenter and all the apparatus being in an adjoining room in front of the subject.

With the pedestrian walking on a non-uniformly lighted street there are two possible factors which may influence his mental state—the varying illumination intensity through which he is passing and the alternating light and dark regions on the street surface before him. To reproduce the first of these a 110-point circular rheostat was placed in series with the lamp illuminating the room. An alternating-current motor, geared down by belts and speed reducers, was arranged to drive a crank. One end of the crank was connected by a shaft with the margin of a wheel of greater radius, so that the latter oscillated through 160 degrees. This was belt-geared to a disc attached to the axle of the rheostat. Thus, when the motor was in operation the illumination in the room where the subject sat was slowly dimmed to a definite point (not, however, to the point where the color of the light changed per-

ceptibly), then increased and this process continued. These conditions roughly parallelled those to which the individual on the street is subjected as he walks along, the brightest point in the cycle in the laboratory corresponding to the position under a street lamp, the darkest part of the cycle to the position midway between lamps and the intervening points in the cycle to the intermediate positions on the street. The speed reducers were combined to produce complete cycles of 25, 55 or 85 seconds.

To reproduce the second factor, the shadows on the street surface, a black box with light interior 115×35×20 cm. with one of the 115×20 edges open was mounted vertically on a table at the subject's right with the open edge facing the left at an angle of 60 degrees to the wall. In strips attached to the edges of this side a piece of cardboard 115×20 cm. with 17×5 cm. holes in it 5 cm. apart could be moved up and down. In the center of the opposite side of the box was a 25-watt tungsten lamp. Thus a slightly fan-shaped array of shadows was cast on the wall directly in front of the subject. The cardboard was weighted at the bottom and attached at the top to a cord which ran through pulleys over the partition into the adjoining room. The cord could be attached to the crank which actuated the rheostat and the shadows thus be made to move up and down at various rates through a distance of 40 cm. This roughly reproduced the slowly changing effect of the light and dark patches on the street surface as the subject walks along under non-uniform lighting.

To parallel the uniform conditions on the street, the room was left at its normal condition with the lamp at maximum intensity. The average brightness on the black wall in front of the subject was then .119 meter candles.[1] When the light was dimmed to the lowest point of the cycle the average brightness was .037 m.c. With the shadows the average figure (averaging maxima and minima of the shadows and of the light spaces between them) was 1.38 m.c. When the shadow equipment was compared with a uniform, the card

[1] Photometric measurements were made over an area of approximately one square meter with a Lummer-Brodhun photometer, using an amyl-acetate lamp as a standard.

in the latter case was drawn up above the box and the light in the box shunted through the rheostat and held constant at a point which gave approximately the same average brightness on the wall. A point was found on the rheostat which gave a brightness of 1.42 m.c. as compared with the 1.38 m.c. for the shadow equipment run at the full current.

II. *Method.*

Experiments were made under these conditions upon various sorts of reaction times and upon attention as involved in a tachistoscopic test similar to that employed in the street experiments. For the latter purpose a portion of the wall between the subject and the experimenter was replaced by a piece of black cardboard 66×100 cm. in the center of which was a piece of ground glass 10 cm. square. Behind this was a rack into which paper or cardboard material could be inserted. Arrangements of geometrical figures in combinations of five, quite similar to those described above (p. 158) were employed. The figures were 4 sq. cm. in area, drawn with a soft pencil on white linen paper. When placed in the rack pressing against the ground glass and illuminated by a lamp 20 cm. distant they were plainly visible to the subject on the other side of the glass. The exposures of one second were made by covering the end of one of the speed reducers with a wooden disc with a sector of proper magnitude removed and arranging a contact of spring brass to press on the disc. The spring contact and the speed reducer were connected to two poles of the lamp circuit which was thus closed during a part of the revolution of the reducer. A telegraph key in the circuit enabled the experimenter to make the exposures whenever he wished. After the exposure the subject reproduced the figures on a small square of paper on the table at his side and placed his drawing in a drawer which the experimenter pushed through the wall for the purpose and which was then withdrawn. In scoring, a figure in the correct position counted 2 and a figure displaced less than a quadrant or disoriented counted 1. A different permutation of objects was shown each trial during the first series on two lighting equipments

in one time order, and then the same material repeated on the two equipments in the opposite time order. That is, each pattern was used once on each of the two equipments under comparison, thus eliminating any error due to degrees of difficulty in the material.

Auditory choice reactions with the feet were sometimes intermixed with the short-exposure tests and sometimes used alone. Keys were made of two pieces of wood $20 \times 10 \times 1$ cm. hinged at one of the longer edges, with brass strips for contacts at the outer edges and held apart by light springs. Each foot rested on one of these keys, the normal weight of the foot being sufficient to close the circuit. The stimuli consisted of a small electric gong and a sounder made by removing the bell from a larger gong so that the armature struck the magnets. The time was measured with two electric vernier chronoscopes. By the use of a double-throw switch, the two chronoscopes could be operated through the same connections and thus could be used in quick succession and counted simultaneously. The counting was facilitated by giving the second stimulus on the 10th, 15th, etc., swing of the first. The chronoscopes were always callibrated at the beginning of the hour. For simple reactions a telegraph key on the table beside the subject was connected in parallel with the foot contacts.

For the stimulus for a simple visual reaction a battery lamp on a frame, so hinged that it could be swung near the ground glass before the eyes of the subject, was placed in parallel with one of the bells by means of a double-throw switch, and a piece of cardboard with a 6 mm. round hole placed between it and the glass. The subject thus received a small patch of light as a stimulus.

In conducting the experiment, two equipments were always employed on a given hour in both time orders, *i. e.*, uniform, non-uniform, non-uniform, uniform, or the reverse. The subject sat passively under the given equipment for approximately 3 minutes prior to each series. On a given comparison half the subjects began with the uniform and half with the non-uniform equipment, and when the same subject worked

two different hours on a given comparison, the second hour began with the equipment the opposite of that of the first hour.

The instructions for the short-exposure test (after a preliminary explanation of the material and observation of a sample exposure) were, "Reproduce as many of the figures as possible in the correct position." Those for the choice reaction were "When you hear the bell lift the right foot and when you hear the sounder lift the left." When the reactions were intermixed with the short-exposures the instructions were a combination of these two: "When you hear the bell lift the right foot, when you hear the sounder lift the left, and when you see the pattern reproduce as many of the figures as possible in the correct position." For the simple reactions the subject was instructed: "Motor reaction to the bell" or, "Motor reaction to the light." The instructions were repeated before each of the four series during the hour. At the beginning of the hour three patterns and ten reactions of the sort to be used were given for warming up, and the first two reactions of the four series were omitted from the results.

The number of trials varied according to the combination of tests used on a given hour. The tachistoscope tests were always given in series of 40, *i. e.*, 20 on each equipment. When choice reactions were used in connection with the tachistoscope there were 80 trials, 40 with the short exposures intermixed and 40 without. With simple reactions it was possible to give 240 per hour, usually 120 auditory and 120 visual.

The reactions and exposures were given at irregular intervals. The order for the choice reactions was determined by shuffling cards containing the same number of right and left at the beginning of each series. When the reactions were intermixed with the tachistoscope, cards marked "T" were intermixed with the others before shuffling.

The subjects were graduate students in the Harvard Laboratory and a few undergraduates doing advanced work in psychology. None of them knew the trend of the results of the experiments on the street. Ten subjects participated at different times.

III. *General Results*

Various combinations of uniform and non-uniform illuminating conditions were studied comparatively and the results tend in general to substantiate those obtained on the street. This can best be shown by averaging together all those series in which a distinctly non-uniform condition was compared with a uniform. Table III summarizes the larger part of such results. The Roman numerals at the left indicate the equipments compared and are as follows:

I.	Non-uniform......	26 sec. dimmer.
	Uniform..........	Dark wall.
II.	Non-uniform......	26 sec. dimmer—shadows stationary.
	Uniform..........	Dark wall.
III.	Non-uniform......	26 sec. dimmer—26 sec. shadows.
	Uniform.........	Dark wall.
IV.	Non-uniform......	26 sec. dimmer—26 sec. shadows.
	Uniform..........	Light wall.
V.	Non-uniform......	55 sec. dimmer.
	Uniform..........	Dark wall.
VI.	Non-uniform......	55 sec. dimmer—shadows stationary.
	Uniform..........	Light wall.
VII.	Non-uniform......	55 sec. dimmer—55 sec. shadows move.
	Uniform..........	Dark wall.
VIII.	Non-uniform......	85 sec. shadows.
	Uniform..........	Light wall.
IX.	Non-uniform......	85 sec. dimmer—85 sec. shadows.
	Uniform..........	Light wall.
X.	Non-uniform......	Shadows stationary.
	Uniform..........	Light wall.
XI.	Non-uniform......	25 mm. shadows stationary.
	Uniform..........	Light wall.

The first column under each heading gives the average per cent. of superiority of the non-uniform equipment (*i. e.*, shorter reaction time or higher score on the tachistoscope, per cents. reckoned in terms of the smaller of the two averages), for the given comparison. Below this figure is in each instance the total number of subjects who yield differences in favor of the non-uniform equipment followed by the total number of subjects who participated on that comparison. The second column gives the average obtained by dividing the differences by the probable error for the various subjects,

TABLE III

	Tachistoscope			Auditory Choice With			Auditory Choice Without			Simple Auditory			Simple Visual		
	% Superiority Non-uniform	Difference / P.E.	M.V. Uniform / M.V. Non-uniform	% Superiority Non-uniform	Difference / P.E.	M.V. Uniform / M.V. Non-uniform	% Superiority Non-uniform	Difference / P.E.	M.V. Uniform / M.V. Non-uniform	% Superiority Non-uniform	Difference / P.E.	M.V. Uniform / M.V. Non-uniform	% Superiority Non-uniform	Difference / P.E.	M.V. Uniform / M.V. Non-uniform
I	2.16 8/11	1.27	.99	.94 6/11	1.63	1.00	1.35 8/11	1.60	.96	.90 3/5	1.46	1.04	7.07 4/5	3.16	1.07
II	−2.50 2/6	2.59	1.04	9.05 5/6	3.03	1.14	6.86 5/6	2.51	1.06	...	...	...	...	...	...
III	−2.30 4/8	1.22	1.14	2.21 5/8	1.55	1.10	2.28 3/8	2.53	.95	...	...	...	...	...	...
IV	...	...	...	...	...	...	...	...	...	5.75 2/2	2.15	1.00	2.70 1/2	3.20	1.30
V	5.79 7/11	1.62	1.08	.76 8/11	1.55	.95	6.36 6/8	1.87	1.12	...	...	...	...	...	...
VI	...	...	...	...	...	...	...	...	...	5.36 5/6	2.22	.95	6.46 5/6	3.88	1.20
VII	...	...	...	...	...	...	2.12 4/5	1.45	1.11	5.01 5/5	1.27	1.15	...	...	...
VIII	...	...	...	...	...	...	2.86 4/6	1.64	1.02	8.65 4/6	3.26	1.04	...	...	...
IX	...	...	...	...	...	...	3.35 3/4	1.56	.92	5.55 7/8	2.49	1.11	−6.10 1/4	2.68	.98
X	4.64 4/9	1.58	.95	2.37 2/4	.91	1.68	.83 7/14	1.42	1.10	7.99 9/10	2.80	1.15	...	...	...
XI	.52 2/4	1.02	.93	...	...	...	...	...	...	3.35	1.53 3/4	.95	...	...	...
Average	1.91 27/49 55%	1.52	1.01	2.50 26/40 65%	1.75	1.09	2.88 40/62 65%	1.72	1.04	5.76 38/46 82%	1.73	1.07	3.24 11/17 65%	3.30	1.12

and the third gives the average of the figures obtained by dividing the per cent. mean variation of the uniform series by that of the non-uniform for each subject. To illustrate: the figures at the beginning of the table indicate that for the tachistoscopic tests on the comparison denoted as I., of the 11 subjects, 8 yield differences in favor of the non-uniform equipment with an average superiority of the non-uniform (including negative values) of 2.16 per cent. The average of the 11 differences divided by their probable error is 1.27. If the per cent. mean variation of the results on the uniform equipment is divided by that for the non-uniform the average of these figures is .99.

The summaries at the bottom of the columns give in the first row the weighted averages of all the first rows above, obtained by multiplying each average by the number of series involved and dividing the sum of these by the total number of series. In the second row of the summary are the sums of the corresponding figures above, *i. e.*, the number of series yielding differences in favor of the non-uniform, the total number of series and the per cent. which the former is of the latter.

From the averages it is evident that there is a general tendency toward greater efficiency under non-uniform illumination. Simple auditory reaction time is superior under such lighting in 82 per cent. of the series an average of 5.8 per cent. Simple visual reaction time is quicker in 65 per cent. of the series, an average of 3.2 per cent. Auditory choice reactions with tachistoscopic trials intermixed yield similar results in 65 per cent. of the series with an average superiority of 2.5 per cent. Auditory choice reactions without the tachistoscopic trials tend in the same direction in 65 per cent. of the series, an average of 2.8 per cent. And the tachistoscopic experiments tend likewise in favor of non-uniform illumination in 55 per cent. of the cases with an average superiority of the non-uniform of 1.9 per cent. The differences in general are between 1.5 and 2 times the probable error of difference and in the case of the visual reactions over 3 times. The mean variations do not show as marked a

tendency, although they average in each method slightly in favor of the non-uniform condition. It might be noted that the individual averages in the simple reactions are based on a larger number of trials and the differences prove greater.

At the end of each hour the subjects were asked whether they had any introspection as to their comparative mental state under the two systems of illumination. In a large majority of the series (83 per cent.) the subject had nothing of interest to report. The usual statement was: "Felt no difference under the two systems"; or "Think I did as well under one system as the other." It is evident that an individual may be influenced by the illuminating conditions without being aware of it. However in the cases in which introspection was given there is no unanimous tendency. Only two of the subjects are at all consistent in their reports, the others feeling more efficient sometimes under uniform and sometimes under non-uniform lighting. There is no general tendency in the introspective as in the objective results. However the two correlate in about two thirds of the individual cases in which introspection is given—a report that the subject is less efficient under a given system corresponding to a slower reaction time. There are no marked individual differences, *i. e.*, subjects who are consistently superior under uniform or non-uniform conditions. On the whole, considering the few cases in which introspection was forthcoming and its variable character, the principal stress must be placed on the objective results. It is evident that a subjective estimate of the mental state under various conditions of illumination is sometimes unreliable, and furthermore such illuminating conditions frequently influence the reactions without producing any conscious effect at all.

Thus the results obtained in the laboratory tend to corroborate those found on the street although not as marked. Reactions are quicker and a higher degree of attention is maintained under non-uniform illumination. It would seem that the results could be attributed solely to the non-uniformity factor either in the form of the increasing and decreasing intensity or of the shadows of the visual field. External

factors such as traffic and extraneous light from store windows were not present in the laboratory. Nor could anything be attributed to the type of lamp or the number and mounting of lamps. Further, when a shadow equipment was used, the brightness of the wall in the corresponding uniform equipment was in some series equated to the average brightness of the former. The results are evidently due to the uniformity factor.

IV. *Correlation of Other Reactions with Auditory Choice*

In the experiments on the street auditory choice reactions were used almost entirely. From Table III, it is evident that in the laboratory simple auditory reactions as well as auditory choice yield greater efficiency under the non-uniform illumination. Data is available for 20 series in which the two were measured on the same hour with the same subjects. If the individual per cents. of superiority of the non-uniform equipment are ranked for the simple and the choice reactions, the two correlate by the method of rank differences to give a coefficient of $.42 \pm .13$. It is to be noted further that simple visual reactions also tend on the average in the same direction as the others. Scarcely enough series are available in which auditory and visual were measured on the same hour to compute correlation on the basis of individual averages, but the general tendency is manifest.

V. *Degrees of Uniformity*

Given the general superiority of non-uniform illumination, the question arises as to various degrees of non-uniformity. In the laboratory as on the street no conclusive answer can be given. In Table III. are a few cases in which results are available under two comparisons that differ merely in their temporal aspect. Combinations I. and V. compare the 26 and 55 sec. dimmer respectively with the constant conditions. The 55 sec. appears considerably superior in tachistoscope experiments and in auditory choice reactions alone, and slightly inferior in auditory reactions intermixed with the tachistoscope. Combinations III. and VII. compare 26 and 55 sec. cycles of the dimmer and shadows moving together,

with the constant conditions. The average difference in auditory choice reaction is slight, but a larger per cent. of the series yield superiority of the non-uniform system in the case of the 55 sec. cycle. The 85 sec. cycle (IX.) of dimmer and shadows, compared however with the light wall equipment, is superior to either the 26 or the 55 sec. cycles. One might also note the comparative results under combinations II. and III. in both forms of choice reaction. With the 25 sec. dimmer and stationary shadows the score is high, but when the shadows move in 26 sec. cycle as well as the dimmer, the score is low. Evidently the rapidly moving shadows offer a distraction.

Some series were performed to test this problem directly. The subjects were given simple auditory and visual reactions under the dimmer moving at three rates on a given hour with the time-error properly controlled and at another time under the shadows moving at three rates. The results indicate that the slower rates are more advantageous with the shadows and the more rapid rates with the dimmer for auditory reactions, while visual reactions are facilitated by the more rapid rates with both.

The slower rates with the dimmer and shadows correspond roughly to the more non-uniform conditions on the street, *i. e.*, to a system in which the subject walking along is less frequently under a lamp and in which the light and dark patches on the street are broader and hence produce a less rapid change in the visual field. There are slight indications in the results on the street of greater efficiency in auditory reactions the more non-uniform the lighting. In the laboratory from the standpoint of the changes in the visual field, auditory reaction is likewise quicker the more non-uniform the system, but from the standpoint of change in illumination intensity this is not the case.

On the whole no definite conclusions can be drawn upon the present problem. It seems best to conclude on the basis of both street and laboratory experiments that within reasonable limits no marked differences in efficiency are found under various degrees of non-uniformity of illumination,

but that the important point is the general superiority of the various non-uniform equipments to the uniform.

VI. *Voluntary Alertness*

The question was raised in the street experiments as to whether the superiority of reaction under non-uniform illumination was not due to a voluntary alertness produced by the necessity of watching more carefully for obstacles in the darker regions between lamps. It was endeavored in the laboratory to answer this question as to whether voluntary attention to one stimulus under the various illuminating conditions would quicken reaction to another. For this purpose auditory choice reactions were given when the subject's attention was somewhat focused on the patch where the tachistoscopic material was to appear, followed by auditory choice reactions with no exposures intermixed. Series were performed on five different illumination comparisons, numbers I., II., III., V. and X. (see p. 173). 80 trials were given on a comparison. The results for the uniform and the non-uniform equipments were then evaluated separately and the efficiency of auditory choice reactions presented alone compared with that of such reactions with tachistoscopic trials intermixed. The former were found to be superior with the majority of the subjects under all the lighting equipments thus investigated. Considering the totals under the five uniform equipments, 76 per cent. of the series yielded a superiority of the reactions presented without the tachistoscope, with an average superiority (including negative cases) of 5.2 per cent. and the average difference 2.44 the probable error of difference. Considering the totals under the five non-uniform equipments, 76 per cent. of the series likewise yielded a superiority of the reactions presented without the tachistoscope with an average superiority of 7.9 per cent. and an average difference 2.6 the probable error. Evidently voluntary alertness to one stimulus under the various illuminating conditions does not quicken reaction to another stimulus.

VII. *Shadows vs. Changing Illumination Intensity*

Finally there remains the question as to whether the effect of non-uniformity on the street is due primarily to the changing illumination through which the subject passes as he walks along the street or to the light and dark patches on the street surface. One series was performed to test this directly, auditory choice and simple reactions being given under the 55 sec. dimmer and under the stationary shadows. Choice reactions proved quicker under the dimmer with 3 of the 5 subjects, with an average superiority of 9 per cent. and simple reaction was quicker for 3 subjects under the shadows with the average difference small. Nothing in the introspection throws light on the matter.

In Table III. if we consider the simple auditory reactions we find the highest comparative efficiency for the non-uniform lighting on comparisons VIII. and X. in which the shadows are alone at the 85 sec. cycle and stationary respectively, but the results with a combination of the shadows with the dimmer are not greatly inferior to these. On choice reactions without the tachistoscope the highest scores are for II. and V. which involve the 26 sec. dimmer with stationary shadows and the 55 sec. dimmer respectively. In these cases, however, the uniform system used involves the dark wall with a possibility of the influence of the brightness variable. Of the others the highest score is for the 85 sec. dimmer with the shadows moving (IX.), which is superior to that for the shadows alone either stationary (X.) or in an 85 sec. cycle (VIII.). Choice reactions intermixed with the short exposures show the highest score for II. a combination of the 26 sec. dimmer and stationary shadows and the next highest for X., the stationary shadows. The tachistoscope shows a high score for both V. and X., the 55 sec. dimmer and the stationary shadows. On the whole, non-uniform illumination produced by the dimmer seems slightly superior to that produced by the shadows in its effect upon choice reaction, while the result is reversed with simple reaction, and a combination of dimmer and shadows seems in general to produce better results then either alone. Presumably the effect of

non-uniform street illumination upon attention and reactions is due to both factors, the changing illumination intensity through which the individual passes and the dark regions on the street surface.

D. Conclusions

The results of experiments upon subjects walking under actual conditions of street illumination indicate rather consistently that the auditory choice reaction time is quicker, the attention more alert and the efficiency in motor coördination greater under a distinctly non-uniform illumination than under a distinctly uniform. There is no other variable that follows as directly with the results as does the uniformity variable. Sometimes the non-uniform street has more distracting elements in the form of people on the sidewalk or extraneous light from stores, sometimes less; sometimes its average illumination intensity is greater, sometimes less; sometimes it has a different type of illuminating unit, sometimes the same; sometimes it constitutes the first of the series, sometimes the second; sometimes it has fewer lamps with different mounting and spacing, sometimes the same number of lamps with the same mounting and spacing. But under all these conditions it shows a superiority over the uniformly lighted street. When the uniformity factor is a variable in the comparative study of two equipments, there is a marked difference in results, and when it is a constant the difference is slight. Under a distinctly non-uniform street illumination as compared with a uniform, the subjects show in 77 per cent. of the series a superiority in motor coördination with an average (including negative values) of 3.6 per cent. superiority; in the tachistoscopic test they are superior in 84 per cent. of the series with an average (including negative values) of 8 per cent.; and in auditory choice reaction every series yields a quicker reaction time with an average of 17 per cent. superiority.

With the problem transferred to the laboratory and non-uniformity produced by slow rhythmic increasing and decreasing of the illumination intensity of the room, or by shadows to break the monotony of the dark wall—conditions

designed to parallel respectively the changing illumination intensity through which the individual walks on a non-uniformly lighted street, and the shadows on the street surface—the results tend in general to corroborate those just mentioned. Further, simple visual and auditory reactions correlate with choice reactions in this tendency. The superiority of reactions under non-uniform lighting on the street cannot be attributed to a voluntary alertness produced by the necessity of watching more carefully for obstacles in the darker regions between lamps, for the experiments show that voluntary attention to an expected stimulus under such illuminating conditions does not quicken reaction to another stimulus. Within reasonable limits no definite relation exists between degree of efficiency in reaction and degree of non-uniformity in illumination, the important point being the general superiority of the various degrees of non-uniformity to the uniformity. The superior effect of the non-uniform street lighting seems due to both the changing illumination intensity through which the individual walks and the light and dark regions on the street surface.

Finally the practical implications of the experiment should be borne in mind. Safety on the street depends largely on the ability to apprehend a dangerous situation and carry out quickly the proper psychomotor response. If external conditions produce a lower degree of attention and a retardation of reaction, the danger of such conditions is manifest. The above experiments show that uniform street illumination as compared with a moderately non-uniform, produces such a state in the case of the pedestrian walking through the street. The work in the laboratory with various degrees of non-uniformity would seem to indicate a similar condition in the case of more rapid movement through the street such as that of the driver or chauffeur. Driver and pedestrian alike must make quick decisions and reactions and a difference in reaction time of a few hundredths of a second may avert or precipitate an accident. The conclusion thus seems warranted that, ceteris paribus, non-uniform illumination is more conducive than uniform to safety on the street.

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FACTORS WHICH INFLUENCE THE AROUSAL OF THE PRIMARY VISUAL MEMORY IMAGE

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I. INTRODUCTION

The development of the problem of individual differences in imagery subsequent to the time of Galton has led away from the original simple solution to an increasing realization of the complexity of the whole matter. We note the failure of investigators to find simple types and the disputes as to the actual criteria of type. Fernald has recently stated that "individual differences in imagery are too complex to be stated in terms of differences in type unless this type is carefully explained in the individual case,"[1] and suggests that perhaps "individual differences may be more profitably stated without the incubus of types,"[2] while Lipmann[3] thinks that a further differentiation is necessary within the visual according as individuals best apprehend (auffassen) hue, saturation, brightness, size, or position.

In view of the present rather unsatisfactory status of the traditional problem of types, and of the nevertheless patent fact that individuals differ markedly in their mental imagery, the present investigation proposes to approach a limited aspect of the question within the field of visual imagery. The effect of certain fairly controllable factors connected with a visual

[1] M. R. Fernald, The Diagnosis of Mental Imagery, *Psychological Monographs*, XIV., 1912 (No. 58), 130.

[2] *Ibid.*, 21. [3] O. Lipmann, Visuelle Auffassungstypen, *Bericht IV Kongress für experimentelle Psychologie*, 1911, 198.

stimulus upon the immediate arousal of visual imagery of that stimulus was studied. Complexity of contour, size, length of exposure of an object, interest aroused in an object, motor reinforcement by tracing, motor distraction and mental distraction during fixation were selected for investigation and their effect upon imagery of the object was determined. These are only a few of the many variables that might be made the subject of profitable study.

The determination of imaginal types in the present case is wholly incidental. The interest of the experiment is four-fold:

1. How far do the above factors, operative during a period of visual stimulation, influence, in general, the arousal of imagery of the stimulus?

2. How far do individuals differ quantitatively in their susceptibility to these various factors?

3. How far do individuals differ qualitatively in the means by which these factors operate in influencing the arousal of visual imagery?

4. How far, if at all, are these factors interrelated?

Two previous experiments have dealt specifically with a few of the above factors. Meakin,[4] working with pairs of plane figures and noting the subsequent rivalry in imagery, finds that the larger or longer-exposed object or that with a notched contour persists in consciousness during a greater part of a given interval than does a simple object. Murray,[5] working with single exposures of simple figures, finds no correlation of duration or excellence of reproduction in imagery with complexity and size.

The other factors involved in the present investigation have been brought out only incidentally in previous experimental studies. Meakin[4] mentions interest as a cause for the more frequent occurrence of images of certain figures. Kuhlmann[6] using pictures of familiar objects as stimuli for imagery, concludes that the tendency for the image of a picture to become that of a real object is due to the fact that there is "more interest and emotional coloring to objects than to pictures." Martin[7] notes that, according to the introspection, the duration of imagery frequently depends on interest.

[4] F. Meakin, Mutual Inhibition of Memory Images, *Harvard Psychological Studies,* I., 1903, 235-275.

[5] E. Murray, Peripheral and Central Factors in Memory Images of Visual Form and Color, *American Jour. of Psychol.,* XVII, 1906, 227.

[6] F. Kuhlmann, Memory Consciousness for Pictures of Familiar Objects, *American Journal of Psychology,* XVIII, 1907, 420.

[7] L. J. Martin, Die Projektionsmethode und die Lokalisation Visueller und anderer Vorstellungsbilder, *Zeits. für Psychologie,* LXI, 1912.

The motor element in visual imagery is repeatedly emphasized by Meakin and he concludes that the factors which he studied (such as size, complexity, broken lines, etc.) are the "conditions which determine the energy diversity, complexity and definiteness of the active process involved in the bestowal of attention upon its object, and . . . such active processes are as essential in ideation as in perception."[8] Kuhlmann, using meaningless forms which are memorized and recalled after an interval, finds motor tendencies in the imagery and these are in some cases "real aids to recall."[9]

Meakin mentions attention in the above quotation. Slaughter[10] notes that the distribution of attention in imagery and stimulus are similar. If a row of dots is followed across with the eyes, the dots can be brought out successively in imagery. Martin[7] states that, according to the introspection of the subjects, the duration of imagery often depends upon attention. Distraction has sometimes been employed in tests of imaginal type. The mode of distraction (visual, auditory, verbal) which produces the greatest effect upon the objective results of a learning "Aufgabe" is supposed to indicate that the subject belongs to the corresponding type. The method has been used with considerable success by some investigators, but it is sometimes found that the distraction has no effect upon the imagery.[11] In the present experiment, however, the method is somewhat different,—the distraction being employed during the stimulus period and the effect upon *subsequent* imagery noted.

II. Apparatus and Method

Each of the seven variables above mentioned was studied, in the present investigation, from three standpoints.

1. *Single* exposures were made of simple geometrical figures, some trials involving and others not involving the given variable. The time necessary for arousal and the time of involuntary holding of subsequent imagery under the former condition was compared with that under the latter.

2. *Simultaneous* exposures were given of two figures, one of which involved the variable under consideration. The rela-

[8] F. Meakin, Mutual Inhibition of Memory Images, *Harvard Psychological Studies*, I, 275.

[9] F. Kuhlmann, Mental Imagery and Memory of Meaningless Forms, *Psychological Review*, XIII, 1906, 344.

[10] J. W. Slaughter, A Preliminary Study of the Behavior of Mental Images, *American Journal of Psychology*, XIII, 1902, 535.

[11] *Cf.* J. E. Downey, Central Processes in Modified Handwriting, *Psychological Monographs*, IX, 1908 (No. 37), 99.

tive predominance of the two in imagery was noted, and compared with the results of the check series in which neither figure involved the variable. The times of arousal and holding of the imagery under the two conditions were compared.

3. The *successive* exposure of two simple figures in check series usually yielded a marked predominance of the second in subsequent imagery. By introducing a hypothetically reinforcing variable on the first object or a distracting one on the second, the effect in reducing the predominance of the second could be noted. The times of arousal and holding of the image in the crucial and check series were compared.

The variables were controlled by the nature of the stimulus object or by the " Aufgabe " during the stimulus period. Figures with notched edges, with twice the usual area, with longer exposure, or figures representing in outline certain meaningful objects, were used to control complexity, size, exposure length and interest respectively. Motor reinforcement consisted in the subject's tracing a figure on the table while fixating it. Motor distraction was produced by writing extraneous words during fixation. Mental distraction consisted in performing mental addition while observing the figure.

An experiment of this sort necessitated an exposure apparatus that would provide a means of eliminating sensory after-images by a flash of light prior to the mid-period, and that would accurately control the time relations of the stimuli.

A few of the earlier series were performed with a relatively crude apparatus, Hering's *Nuancierungsapparat* adapted for the purpose. This consisted of a box 94 x 30 x 20 cm. with one of the broader sides open, and mounted vertically with the open side facing a north window. The subject sitting on a high stool with his forehead on a rest, could see, through a small aperture in the top of the box and through a diagonal piece of glass, the black rectangular field at the bottom. When the experimenter pulled a string, a black cover fell across this field and automatically opened a door on the side of the box which admitted light reflected from a piece of milk glass outside. This light was reflected upward from the diagonal piece of glass in the box to the eye. The stimulus object of black cardboard was placed on the field and at the signal " ready-open," the subject opened his eyes and fixated the object for 5 seconds until the experimenter, noting a stop-watch, pulled the string. As soon as the flash appeared the subject closed his eyes and observed the imagery that arose, signalling its appearance and disappearance by raising

and lowering the finger. The experimenter took the time with a stop-watch.

The majority of the work, however, was performed with an enlarged and somewhat modified Dodge tachistoscope.[12] This consists in principle of a box with black interior with two exposure fields at opposite corners visible through an opening at the third corner,—one field, when illuminated, seen directly through a diagonal piece of smoked glass and the other, when illuminated, seen reflected from the glass which with dark background acts as a mirror. The present apparatus was 25 cm. deep with the 25 x 25 cm. fields 96 cm. from the rectangular opening hooded for the eyes of the subject. The fields were illuminated by light from two 25 watt tungsten lamps reflected by mirrors outside the range of vision. A third lamp was placed inside the apparatus just to the right of the smoked glass in such a position that its filaments were thrown out of focus on the eye by a 10 cm. condensing lens. A slit 70 x 8 mm. in a cardboard screen admitted a small portion of the light from the filaments. This was sufficiently strong to destroy the after-image, but not sufficiently so to cause discomfort. The lamps were on separate circuits and by proper control the stimulus objects appeared successively in the same apparent position followed by the flash of light. The apparatus rested on legs on a table with the hood on the level with the eyes of the subject sitting on an adjustable chair. The experimenter sat at one side of the apparatus at a table containing the switch-board, stop-watches and material for stimuli.

A slit was cut in the cover of the apparatus above each of the exposure fields to admit the insertion of pieces of 25 x 25 cm. cardboard with the stimulus objects pasted or drawn upon them. One field could be reached from the experimenter's seat, but the other could not without standing up and reaching across the apparatus. At this latter were installed four traveling frames of light brass suitable for holding stimulus cards. Their contiguous surfaces were perfectly smooth and they were operated from the experimenter's seat by strings and pulleys so arranged that any frame could be pulled into position and the others drawn to one side. The field was masked so that no brass was visible. The frames were loaded at the beginning of the hour's work and it was thus possible to avoid much confusion and save considerable time during the series.

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[12] *Cf.* R. Dodge, An Improved Exposure Apparatus, *Psychological Bulletin*, IV, 1907, 10-13.

The length of exposure of the stimuli was controlled by means of the circuits through the lamps. A wooden disc 22 cm. in diameter, mounted on an axis, was driven at the rate of 3 revolutions per minute by an alternating current motor reduced by belt gear. On this disc were mounted three brass contacts in the form of arcs of circles of different radii having the axle as the common center. The radius drawn to the end of one arc marked the beginning of the next, so that, as the disc revolved, they would in succession pass a given radius. Three contacts of spring brass, with their free ends in the same line were adjusted to press in succession upon the contacts on the disc. Thus during a part of the revolution the first spring contact was on the first contact on the disc; just as it reached the end of the arc and pressed against the wood, the second spring contact pressed on the second contact on the disc, etc. The contacts on the disc were connected with the axle and mounting and thus to one pole of the lamp circuit. The three spring contacts led through the three lamps respectively and joined at the other pole of the main line. Thus the three lamps were lighted in succession, one disappearing just as the next appeared. The first arc on the disc was stationary and of the proper length to give a 6 second exposure. The second was pivoted at the end nearest the first and held in position by friction in a narrow slit sawed in the wood along the arc. The length of the contact could be shortened by pushing the free end farther through the disc so that the spring that rested on it would touch merely the wood. The third arc was of the proper length to give a one-second exposure and was soldered to a strip of brass passing under a screw clamp at the axle so that it could be moved along in a shallow groove to a position immediately following the end of the second contact. Thus the two objects in the tachistoscope could be shown successively for various lengths of time followed by the flash of one second. When it was desired to use single exposures a switch in the circuit of the first lamp was opened. Other switches made it possible to reverse the order in which the first two lamps were lighted or to have one lamp lighted by both of the first two contacts. In part of the work, the motor and lamp-controlling mechanism set on a table with the tachistoscope, but later they were moved to an adjoining room and the wires led through the wall. The sound of the motor was never noted as a distraction by the subjects and furthermore it was a constant factor.

To record the time of arousal and holding of imagery two stop-watches were used. One was of the football type, started

and stopped by plungers at the side. It was mounted with two short strips of brass, each pivoted at one end resting on the respective plungers. The free ends were connected by light steel wires through screw eyes to the armatures of two telegraph sounders. These latter was actuated by a make-and-break key operated by the subject. With the key in its normal position the sounder which pulled the lever against the "stop" plunger was in operation. When the subject pressed his key it broke that circuit and closed one through the other sounder which actuated the "start" plunger. When the key was released the original circuit was again closed. This watch was used to record the time of holding of the image. The other watch was of the ordinary stem starting type and was mounted horizontally with a strip of band iron pivoted a short distance to one side of the stem. Opposite the end of this was a horseshoe electromagnet which could be operated by two parallel circuits. The first of these was closed by a contact at the proper point on the rotating disc which controlled the exposures in the tachistoscope,—a piece of brass on the disc brushing across two poles 2 mm. apart. The second circuit was closed by a relay in connection with the sounder which started the first watch. Thus the watch started automatically just as the second object disappeared in the tachistoscope, and stopped when the subject signalled the presence of the image by pressing the key.

The materials for the experiment consisted, except in the series on interest, of simple geometrical or meaningless figures. None of the polygons had more than six sides and the curved figures were comparatively simple. The figures were uniformly of 36 sq. cm. area with the exception of some of twice that area used in the study of the effect of size. Those used in the "*Nuancierungsapparat*" were cut from black cardboard and were exposed upon a field of the same material. Those employed in the Dodge tachistoscope were of white paper pasted on black cardboard, or were drawn in outline with ink on light grey cardboard. Those used in single and successive exposures were centered on the card. Those used in simultaneous exposures were side by side with a space of 2 cm. between their nearest points. In the latter case the space error was always obviated by presenting the two figures in both space orders in different trials. Similarly the time error on successive exposures was controlled,—the same figure occurring first in half of the trials in which it appeared, and occurring second in the other half.

The instructions given to the subjects in previous experi-

ments involving a definite visual Aufgabe have varied considerably. Meakin[13] and Murray[14] instructed their subjects to await passively the entrance of the image into consciousness. Martin[15] and Ogden[16] told theirs to "get an image." Perky's instructions[17] were to be on the lookout for images. In the present experiment the subjects were given two hours of preliminary training in which they were instructed to "call up a visual image of the object just seen, observing it as passively as possible and signalling its presence by pressing the telegraph key and its disappearance by releasing the key." These instructions rapidly passed into a determining set and in all subsequent work, after being told to fixate the figures that appeared or to fixate between them (on simultaneous exposures) or to perform the required operation during fixation, the subject was merely instructed to "close the eyes after the flash and observe whatever imagery arises, pressing the key when the image appears and releasing it when the image disappears." These instructions were given at the outset of the hour's work and not repeated during the series. After each trial the subject described the image and the introspection was recorded verbatim.

Visual stimuli seemed more suitable for an experiment of this sort than word stimuli, for they afford a better objective control of the variables under investigation. Word stimuli would in the present case be more liable to introduce extraneous factors such as suggestion. Moreover the visual stimulus affords a better control of the subject's state during the foreperiod, for he is always observing a figure for 5 or 6 seconds prior to the mid-period in which the image appears.

That after-images did not play a part in the experiment seems evident from the intensity of the flash that followed the exposures. The subjects frequently noted a momentary after-image which was "wiped out" by the flash. Furthermore the qualitative aspect and the temporal course of the images for a given subject were fairly similar whether the

[13] F. Meakin, Mutual Inhibition of Memory Images, *Harvard Psychological Studies*, I, 1903, 237. [14] E. Murray, Peripheral and Central Factors in Memory Images of Visual Form and Color, *American Journal of Psychology*, XVII, 1906, 229.

[15] L. J. Martin, Die Projektionsmethods und die Lokalisation Visueller und anderer Vorstellungsbilder, *Zeitschrift für Psychologie*, LXI, 1912, 329. [16] R. M. Ogden, Experimental Criteria for Differentiating Memory and Imagination in Projected Images, *Psychological Review*, XX, 1913, 379.

[17] C. W. Perky, Experimental Study of Imagination, *American Journal of Psychology*, XXI, 1910, 428.

stimulus was a white figure on a black field, a black cardboard figure on a black ground or a figure drawn in outline with ink on a grey card.

In some of the previous work of other experimenters the imagery has been observed during a definite interval,—frequently a minute,—the successive appearances of the image being recorded. The present work dealt only with the primary visual memory image. Subsequent recurrences after the first disappearance of the image were not taken into consideration. In one series the times for the primary and for the recurring image (i. e. the total time the image was present during a minute) were recorded for four subjects,—about 60 trials each. The coefficients of correlation between the two times by the Pearson product-moments formula were:

$$.6056 \pm .063$$
$$.4710 \pm .089$$
$$.4298 \pm .089$$
$$.4004 \pm .075$$

If as satisfactory results can be obtained by using the primary image which lasts from 1 to 20 seconds as by using the recurring images for a minute, the great saving of time is evident.

The experiments were performed in the Harvard Psychological Laboratory during the academic years 1913-14 and 1914-15. Fourteen subjects participated at various times in the work. Of these one was an instructor in the department, nine were graduate students (two of them women) and three were undergraduates of considerable psychological experience. The entire experiment comprises reports on approximately 5500 images.

The writer expresses his sincere obligations to Professor Herbert S. Langfeld and Professor Hugo Münsterberg.

III. Experimental Results

A. *Complexity of Contour*

The results of the experiments on complexity of contour are summarized in Table I. The first column gives the subject. The next two give the results for single exposures,—the % superiority[18] of the notched series to the simple, i. e., the % by which the average time of holding is greater, and the difference between the two averages divided by the probable error of difference. The next six give the results for simultaneous exposures. The introspective accounts fall readily into trials in which the notched or simple figure predomi-

[18] Percents reckoned in terms of the smaller figure throughout the work.

nates in imagery (i. e., is clearer or more persistent) or in which the two figures are equal. The table gives the % of the trials in which the notched predominates and in which the simple does so. Then follow the % superiority of the notched series to the check series (with both figures simple) in times of arousal and holding, i. e., the % by which the average time of arousal is less and the average time of holding greater, with the respective differences divided by the probable error of difference. The remaining columns give the results for successive exposures. The % of trials in which, according to the introspection, the figure exposed second predominates or appears alone in imagery in the series with the notched object shown first, is divided by the corresponding % for the normal series with both objects simple. A smaller ratio in the table indicates a greater effect of the complex first figure in reducing the natural predominance of the second figure. Then follow the % superiority of the series with the first figure notched to that with both figures simple in times of arousal and holding of imagery, with the corresponding differences divided by the probable error of difference. The temporal aspect of the image in the simultaneous and successive methods is not as important as the qualitative. It merely indicates whether the images rise more quickly and hold longer when one of the objects shown involves the given variable than when both are simple,—a factor measured more directly in single exposures.

The table shows that with 6 of the 7 subjects who participated on single exposures, the notched image holds longer on the average, than the simple, with an average superiority, including the negative case, of 11%. On simultaneous exposures the notched figure predominates in imagery in 69% of the trials as compared with 9% for the simple figure, while in the check series (not given in the table) the majority of the trials yield images that are equal. The images rise more slowly and hold longer on the notched series than on the check series.[19] In successive exposures the second figure predomi-

[19] It may be noted in passing that the whole series of experiments on imagery indicates that the time of holding is a much better criterion by which to judge the influence of various factors upon imagery, than is the time of arousal. The two times were correlated for 10 subjects on about 140 trials each by the Pearson produce-moments formula. There were three appreciable negative coefficients (.23, .30 and .34) and one positive (.29) while the others were small. In many cases the subjects get into a certain rhythm of arousal which is little influenced by the character of the stimuli. The time of holding correlates more often with the introspection.

TABLE I
COMPLEXITY OF CONTOUR

Subject	Single Exposure: Time: Holding: Percentage of superiority notched	Single Exposure: Time: Holding: Difference / P. E.	Simultaneous Exposure: Report: Notched predominate	Simultaneous Exposure: Report: Simple predominate	Simultaneous Exposure: Time: Arousal: Percentage of superiority notched	Simultaneous Exposure: Time: Arousal: Difference / P. E.	Simultaneous Exposure: Time: Holding: Percentage of superiority notched	Simultaneous Exposure: Time: Holding: Difference / P. E.	Successive Exposure: Report: Notched / normal	Successive Exposure: Time: Arousal: Percentage of superiority notched	Successive Exposure: Time: Arousal: Difference / P. E.	Successive Exposure: Time: Holding: Percentage of superiority notched	Successive Exposure: Time: Holding: Difference / P. E.
Br	—3.0	1.6											
Bz			75%	17%	—27.2	3.8	18.9	4.6	.40	16.2	2.7	0	0
C	14.2	1.5											
D			96%	4%	17.4	1.6	64.4	7.9	.12	—8.1	.8	14.1	3.1
F	6.3	.9											
H	15.8	1.1											
L	28.8	1.5	25%	12%	—70.8	14.7	21.0	4.7	.43	—7.1	1.7	64.1	9.8
Ms	2.7	1.0											
Mt	10.0	.8											
R			80%	5%	-290.0	12.8	40.4	7.0	.43	22.7	1.7	4.5	.6
Average........	10.7	1.2	69%	9%	—92.6	8.2	36.2	6.1	.35	5.9	1.7	20.6	3.3

nates in imagery about .35 as frequently when the first figure is notched as when both are simple, and the images in the former case rise somewhat more quickly and hold considerably longer than in the latter.

As a further light on the single exposure method a series was performed in which the visual stimuli were not presented. The subject sat in the same position as in the previous series with closed eyes and after the signal "ready" the experimenter named the object,—"notched square," "simple square," etc. The subject was to call up a memory image of the object as he had seen it a week before and signal its presence in the usual manner. The results tend in the same direction as the above,—the complex images holding longer for 5 of the 7 subjects with an average superiority of 8.5%.

The qualitative aspect of the results in some cases throws light upon the quantitative. There are, of course, individual differences in the color, clearness, position, etc., of the image and in its mode of appearance and disappearance. Such facts, although of general interest, do not concern the present problem. Mention will be made of only those points of introspection which indicate the effect of the variable under investigation.

On single exposures with *Br* kinaesthetic and affective factors seem prominent. The notched figures are frequently reported as "clear and pleasant." The notched circle, however, is inferior in time of holding to the simple and is described as "vague kinaesthesis going around; not as pleasant as the plain circle; little jar as if rolling along and the rolling jerky; sort of kinaesthetic jar." There is also a "tendency to look around it; less of this in the image." *F* occasionally reports associations in connection with a figure that holds rather long in imagery. *H* notes that it is "easier to attend to the notched figure, for there is more complexity." *L* is very kinaesthetic and often thinks of going around the figure or notes a "kinaesthetic image on a visual background" or a figure "filled in with kinaesthesis," or "intimation of teeth on top and kinaesthesis below." This kinaesthetic aspect seems to correlate with the longer holding of the notched image. However in the check series with word stimuli, he experiences great difficulty in getting an image at all of the notched figures. *Ms* notes on the notched circle, "Revolved; associated with pinwheel" or "Associated with circular saw; teeth slanted." This associative factor tends to lengthen the time of the image. In the check series with word stimuli

the reports are somewhat similar to the preceding. *C* and *F* note movement of the eyes about the image of the notched figure. *L,* as just mentioned, has difficulty in calling up images of the notched figures and such images are less clear, slower of arousal and less persistent.

In simultaneous exposures *Bz* sometimes reports only the middle part of the two objects. This would seem to follow with the fact that the subject fixates between the two figures and the inner parts are consequently more in the focus of attention. *D* notes, " I try to divide the attention but I think the ragged one attracts me and the other fades first." *L* also shows a tendency for the portion nearest the point of fixation to be most marked in imagery. *R* states that the " notched ones were interesting."

On successive exposures *Bz* states that the notched ones are more interesting. With *L,* when the first object appears in imagery (as it does only in the notched series), it is frequently in kinaesthetic terms. In a number of trials the second takes on the notched attribute of the first. For example, " second with wavy lines ; " " First, then second inside it crumply."

B. *Size*

The results of the experiments upon the influence of size are summarized in Table II which is identical in form with Table I except for the substitution of " large " for " notched " and " small " for " simple." The table shows that in single exposures the images of the larger figures hold 11% longer on the average than those of the small. On simultaneous exposures the large figure predominates in imagery in 32% of the trials as against 22% for the small figure, while the check series (not given) usually yields both images equal, and the images rise more slowly and hold longer on the series involving large objects than on the check series. On successive exposures the second figure predominates in imagery .76 as frequently when the first figure is larger as when both are of equal size and the images in the former case rise, in general, more quickly and hold longer.

As a further check on the single exposure method a series was performed in which the visual stimuli were not presented. The subject sat in the same position as in the previous series with closed eyes, and after the signal " ready " the experimenter named the object,—" large square," " small square," etc. The subject was to call up a memory image of the object as he had seen it a week before and signal its presence in the usual manner. The results tend to substantiate the above,

TABLE II

SIZE

Subject	Single Exposure: Time: Holding: Percentage of superiority large	Single Exposure: Time: Holding: Difference / P. E.	Simultaneous Exposure: Report: Large predominate	Simultaneous Exposure: Report: Small predominate	Simultaneous Exposure: Time: Arousal: Percentage of superiority large	Simultaneous Exposure: Time: Arousal: Difference / P. E.	Simultaneous Exposure: Time: Holding: Percentage of superiority large	Simultaneous Exposure: Time: Holding: Difference / P. E.	Successive Exposure: Report: Large / normal	Successive Exposure: Time: Arousal: Percentage of superiority large	Successive Exposure: Time: Arousal: Difference / P. E.	Successive Exposure: Time: Holding: Percentage of superiority large	Successive Exposure: Time: Holding: Difference / P. E.
Br	8.0	1.3											
Bz			42%	42%	—15.3	3.3	24.4	12.2	.90	29.8	5.7	24.6	6.9
C	10.4	1.2											
D			4%	4%	1.5	.2	52.0	8.0	.91	19.4	2.2	—20.0	3.4
F	1.3	1.6											
H	44.3	1.0											
L	9.2	2.4	31%	19%	—84.0	16.4	15.7	4.4	.58	—14.0	2.7	57.2	7.1
Ms	10.4	.9											
Mt	—6.5	.6											
R			50%	25%	-178.0	11.5	25.0	5.0	.63	72.2	3.9	—5.3	.8
Average........	11.0	1.3	32%	22%	—68.9	7.8	29.2	7.4	.76	26.8	3.6	14.1	4.5

the image of the larger object holding 10% longer on the average.

A few points from the introspection may be noted. On single exposures with *Br* there is a possible correlation of time of holding with affective tone, the large figures being frequently reported as more pleasant. *C* notes that the large image sometimes " swells up " or " spreads out " at the end. *H* is " conscious of the effort to see the whole image; the eyes seem focussing one part and then another." *L* notes occasional kinaesthesis with the large figures. *Mt* states, " In looking at the object I tend to draw the part that interests me and the image has the heavy black line I have drawn." In the series with word stimuli *C* reports the large images much larger than the original. *F* notes the images as tending to build up. *L* finds the large images more difficult of arousal, e. g., " Mostly kinaesthetic; felt self pushing it out; tendency to make it small first; the pushing seems to be in the eye muscles; larger it is the longer it takes to get it." For *Ms* the large images are much larger than the original, often starting large and growing smaller.[20]

In the simultaneous series *Bz* gets " the smaller figure as a whole, while with the larger it is a process of building up."

On successive exposures *L* notes occasionally the image of the second figure larger than the original. This is doubtless due to the influence of the first figure. He speaks of carrying in mind the difference in size.

C. *Length of Exposure*

The results of the experiments on length of exposure are summarized in Table III. Its form is like that of the preceding tables. The exposures were 5 and 10 seconds for the single exposure method. For the simultaneous, the apparatus was arranged so that one figure appeared alone and then the other beside it. A piece of 6 mm. board 25 x 10 cm. and painted black, was hinged to the edge of the tachistoscope with spring hinges in such a way that when a black thread was held taut one stimulus figure was obscured and when the thread was released the shutter flew back against the wall. The electrical connections were changed so that both contacts on the disc governing the exposures lighted the same lamp of the tachistoscope. The experimenter held the thread taut during the 6 seconds on the first contact and released

[20] It may be noted, as of methodological interest, that word stimuli have a greater effect in emphasizing an attribute of a figure than do the visual stimuli.

it at the slight flicker when the contacts changed. Thus the left stimulus appeared for 6 seconds and then both for 6 seconds. The subject fixated the first object and when both appeared fixated between them.[21] In the table are given the per cent of trials in which the longer exposed of the two predominates and in which the shorter does so, with the per cent superiority in times of arousal and holding of the present series to the check series with the objects shown together an equal length of time. On successive exposures the variable was introduced by showing the second figure for one second and comparing such trials with the normal successive exposures of six seconds each.

The table shows that for single exposures the longer exposed object holds some 10% longer in imagery on the average, although 3 of the 7 subjects show slight negative tendencies. The average results on the simultaneous method are identical for the long and short series. This is due to two subjects, *D* and *L*, whose introspection (infra) indicates the entrance of another factor. The average difference in the temporal aspect of the image under these conditions is slight. On successive exposures the second figure predominates about .81 as frequently when its exposure is shortened to one second as when both are shown for 6 seconds. The times are not given in the table as they show only slight differences in both directions. If, however, the holding time on the 6 and 1 second series is evaluated for the trials in which the first object predominates compared with those in which the second does so, the average of the former is 18% superior to the latter with the differences 2.4 the probable error on the average. In other words the imagery does not hold as long when the object exposed for 1 second predominates as when that exposed for 6 seconds predominates,—a result similar to that found with single exposures.

A few points from the introspection may be noted. On simultaneous exposures *A* frequently gets an "exact reproduction of the original; first one and then both." Evidently the distribution of attention in the image follows that in the stimulus. *Bz* says, "I take in the first one more quickly than the one beside something already in attention; feels as if the

[21] No other method of studying this factor from the simultaneous standpoint seems available. Showing one object alone in the center of the field followed by the same object with another beside it has merely the effect of successive exposures, and the first, when it does appear in imagery, is in the center like the original stimulus. Showing both objects and then cutting out one would leave the effect of recency which is pronounced on all the work on successive exposures.

TABLE III
LENGTH OF EXPOSURE

Subject	SINGLE EXPOSURE — Time — Holding: Percentage of superiority long exp.	SINGLE EXPOSURE — Time — Holding: Difference / P. E.	SIMULTANEOUS EXPOSURE — Report: Long exp. predominate	SIMULTANEOUS EXPOSURE — Report: Short exp. predominate	SIMULTANEOUS EXPOSURE — Time — Arousal: Percentage of superiority long exp.	SIMULTANEOUS EXPOSURE — Time — Arousal: Difference / P. E.	SIMULTANEOUS EXPOSURE — Time — Holding: Percentage of superiority long exp.	SIMULTANEOUS EXPOSURE — Time — Holding: Difference / P. E.	SUCCESSIVE EXPOSURE — Report: Short / normal
A			45%	10%	—29.0	.9	—15.7	1.4	
Br	11.2	2.2							
Bz			100%	0%	.4	.1	—22.5	7.1	.86
C	17.0	1.6							
D			0%	100%	7.8	1.3	21.1	3.7	1.00
F	—5.5	.5							
G									.26
H	36.5	1.1							1.00
K									.46
L	—2.8	.2	15%	77%	—2.6	.5	21.0	2.2	1.03
Ms	17.2	1.6							
Mt	—4.4	.7							
R			49%	22%	6.1	.7	—2.1	.4	1.12
T									.69
Average....	9.9	1.1	42%	42%	—3.5	.7	—.4	3.0	.81

attention swung back to it after closing the eyes; fact it has been there longer turns the attention to it." *D* always gets the shorter exposed figure clearer in imagery. In the early trials it is covered with a "veil," but the image of that one is nevertheless clearer. Toward the end the veil is "not present so much as its effect in making the image blacker and attracting attention to it." Probably the stimulus to attention of the screened objects accounts for the results. "Knowing it is screened arouses curiosity and interest in it when it comes; there is a tendency to go to the veiled object when it appears, although I try to fixate between them." *L* reports a "decided feeling of something coming in; I fixate the first and when I move to the middle it throws the attention to the other side; I feel the attention go." The results of *D* and *L* thus cannot be considered as evidence against the positive effect of exposure length, for owing to the technique of the experiment other variables entered with them,—in the case of *D* the factor of interest in the concealed object which is to appear, and with *L* the overshooting of the motor impulse or attention in turning from one figure to the mid-point between the two.

On successive exposures with *Bz* the effect of shortened exposure is more marked than the table indicates. In the trials where the second figure predominates he states that "it is not as dominant on the shortened exposure; in the normal, the first object appears in imagery only at the end, whereas with the shortened exposure it appears relatively earlier." With *D* the second object is relatively clearer in imagery in the series with shortened exposure,—a result the opposite of the general tendency. The following introspection explains the fact: "During the second exposure I recall the first by kinaesthesis; on the short exposure the first does not have time to come into relation with the second; the kinaesthesis of the first cannot arise during the second as it usually does." *L* reports that with the short exposure the images "come immediately and are more involuntary than with the longer exposure." *K* shows a similar tendency. With *Ms* and *T* one figure carries associations and tends to predominate whether shown first or second.

D. *Interest*

The results of the experiments upon the rôle of interest are summarized in Table IV. Its form is identical with that of Table I with the interesting figure substituted for the complex. The table shows that on single exposures the images

TABLE IV
Interest

Subject	Single Exposure — Time — Holding — Percentage of superiority interesting	Single Exposure — Time — Holding — Difference / P. E.	Simultaneous Exposure — Report — Interesting predominate	Simultaneous Exposure — Report — Other predominate	Simultaneous Exposure — Time — Holding — Percentage of superiority interesting	Simultaneous Exposure — Time — Holding — Difference / P. E.	Successive Exposure — Report — Interesting / normal	Successive Exposure — Time — Arousal — Percentage of superiority interesting	Successive Exposure — Time — Arousal — Difference / P. E.	Successive Exposure — Time — Holding — Percentage of superiority interesting	Successive Exposure — Time — Holding — Difference / P. E.
Br	—15.0	3.4									
Bz			75%	5%	12.2	4.7	.25	—7.5	1.3	3.4	.8
C	.5	.1	45%	28%	—14.8	4.4					
D							.58	—.5	.7	14.6	2.9
F	13.0	1.9	55%	7%	—46.3	11.4					
H	—3.9	.4	10%	2%	20.3	.5					
L	—1.0	.2	68%	16%	9.7	2.6	.21	—6.7	.4	8.1	.4
Ms	40.0	7.9	33%	13%	88.2	13.1					
Mt	—11.8	1.5	42%	12%	—41.0	10.6					
R							.30	56.0	4.8	15.5	4.6
Average..	3.1	2.2	47%	10%	4.2	6.6	.33	10.3	1.8	10.4	2.3

hold somewhat longer for the meaningful figures. Some of these figures, however, were much more interesting than the others. If the results for the three most interesting stimuli are averaged together, they hold 18% longer than the average of the normal series. In simultaneous exposures the interesting figure predominates in imagery in 47% of the trials as against 10% for the meaningless figure, whereas in check series with both figures simple, the majority of the trials yield equal images, and the images hold 4% longer in the former case. On successive exposures with the first object interesting the second predominates in imagery only .33 as frequently as when both are meaningless, while the images rise more quickly and hold longer under the former condition.

The following points in the introspection may be noted. In single exposures associations are frequent in connection with the imagery of all but the two most kinaesthetic subjects *L* and *Mt*. The figures that yield for a given subject the most associations are usually held by him longer in imagery than the other figures. With *F* there is a possible correlation between the time the image holds and the pleasantness of the associations. With *Br, C* and *Ms* the images tend to become the real objects represented instead of the plane figures.

In simultaneous exposures the factor of interest is mentioned by all the subjects. With three, *Br, Ms* and *Mt*, there is a tendency for the images to become real objects. There is frequently a change in predominance of an interesting object as the series progresses and the same object occurs several times. Toward the end of the series the images tend to be more nearly equal. This occurs with five subjects. As *Br* says, " The reason they even up in clearness is because at the beginning there is more difference in meaning. As I go along with it, the one tends to lose some of its associative value through repetition and the other tends to get a meaning. You exhaust the interest of one object and try to get interest for the other." Or as *H* states, " One object attracts the attention more at first, but after becoming familiar with it, it is easier to distribute the attention."

On successive exposures *Bz* states that " the more interesting it is the more it gets into consciousness." *Bz* and *R* have a tendency for the outline figures to become real objects in imagery. There is further a tendency with three subjects for the interest to wear off with repetition and affect the imagery. Considering the successive trials the second figure predominates more frequently toward the end of the series.

TABLE V
Motor Reinforcement

Subject	SINGLE EXPOSURE				SIMULTANEOUS EXPOSURE								SUCCESSIVE EXPOSURE		
	Time				Report		Time						Report	Time	
	Arousal		Holding				Arousal		Holding					Holding	
	Percentage of superiority traced	Difference / P. E.	Percentage of superiority traced	Difference / P. E.	Traced predominate	Other predominate	Percentage of superiority traced	Difference / P. E.	Percentage of superiority traced	Difference / P. E.			Traced / normal	Percentage of superiority traced	Difference / P. E.
Bz	—8.6	2.2	1.3	.2	95%	0%	10.7	2.2	24.4	4.9			.14	97.0	26.0
Bz													.09*	33.3	4.7
D	9.0	1.5	31.0	7.2	100%	0%	—36.2	3.4	10.8	2.0			.04*	2.6	.4
F													1.01	—62.8	12.7
H													1.00	—72.4	16.5
K													.69	—51.3	5.1
L	16.0	3.8	1.2	.3	40%	45%	—5.9	2.2	9.6	1.9			.91	—40.0	6.1
Ms													.08	14.2	4.2
R	—23.0	1.9	31.0	3.2	80%	10%	—260.0	11.1	6.5	.9			.74	—8.5	1.3
R													.42*	13.0	2.1
T													.67	300.0	23.0
Average........	—1.4	2.3	16.1	2.7	79%	14%	—72.8	4.7	12.8	2.4			.53	20.2	9.3

* Check series; performed later.

E. *Motor Reinforcement*

The results of the experiments on motor reinforcement produced by tracing with a pencil on the table the outline of the figure fixated, are summarized in Table V, which is similar in form to the previous tables. On simultaneous exposures the subject fixated between the figures while tracing one of them. The table shows a slight average difference on single exposure between the tracing and the normal series in times of arousal, but a 16% superiority of the latter in time of holding. On simultaneous exposures the traced object predominates in 79% of the trials as compared with 14% for the other object. The images in the tracing series rise much more slowly and hold considerably longer than in the check series with no tracing. The delay of the images on the tracing series is probably due to the confusion of having in the hand the pencil with which the tracing was done, and the necessity of laying down the pencil or signalling with the other hand. On successive exposures with the first object traced the second predominates only .53 as frequently as when no tracing is done, and the images hold 20% longer on the average under the former condition.

The introspection indicates further how the tracing tends to reinforce the imagery. On single exposures *Bz* notes, "Tracing helps; the eyes go around it and the eye muscles repeat in imagery; see the images usually as enclosed spaces but with tracing I see the edge more." "Trace the triangle with the eye on the apex and it comes that way in the image." The distribution of attention in the image thus follows that of the exposure. *D* shows a similar tendency. "Image is as I draw it, e. g., imperfections in the angles I draw." *L* feels "kinaesthesis during the image, mostly in the arms." He further notes a "better control of the image when traced." *R* has a "tendency to trace the image; like eye movement, or as if the line ran around." Again, "Image comes from the tracing; sometimes forget what was shown but remember it by movement of the hand; think the tracing does not add to it except when I forget image and remember it kinaesthetically."

On simultaneous exposures the only reports of interest are those of *L*: "Try to divide attention, fixate between them and trace automatically; this causes alternation of attention between one object and the other and in imagery the figures frequently alternate."

On successive exposures *Bz* frequently reports kinaesthetic imagery in conjunction with the visual, and this kinaesthetic

imagery is of the first object. He further adds, " It helps to work the kinaesthetic and visual together; the kinaesthesis brings up the visual; it fixes the outlines better; the eyes go around more on the tracing although they sometimes do so on the normal." *D* tends to " have an idea of the first object in kinaesthetic terms while looking at the second." This tendency has been noted by *D* in other series. In the present case it is more marked on the tracing series than on the normal. With *H* the second object always appears alone. She shows here and elsewhere a pronounced tendency toward immediate arousal of imagery of the object just seen,—a marked visual perseveration. *K* notes a change from a visual to a motor attitude. " Tracing often lessens interest in looks; there is a change of attitude in some figures it rules out the bad parts while others I do not like unless I trace them." *L* finds occasionally a " motor memory of the first while looking at the second,—a thought of the movements of the hand." *Ms* occasionally reports kinaesthetic imagery in conjunction with the visual and in general " likes to trace them." *R* sometimes finds that " kinaesthetic images bring up the first object." He finds that " tracing concentrates the attention."

F. *Motor Distraction*

The results of experiments on motor distraction are summarized in Table VI. The distraction was produced by writing on the table extraneous words of three or more syllables during fixation. The word was given before the object appeared on single exposures and one second before the second object appeared on successive exposures, and the subject wrote and rewrote the word during fixation. It was not possible to test the factor of motor distraction directly with simultaneous exposures because the distraction would naturally involve both objects. The best approach to the problem seemed to be to direct the motor attitude to the objects alternately and see if in the subsequent imagery the attention would follow a similar course. Tracing one object would serve as a distraction from the other and so there would be an alternating distraction and an alternating reinforcement. The experimenter said, " Right, left, right, left " during the exposure in time with the swings of a pendulum properly adjusted, starting alternately with right and left in successive trials. The subject traced on the table the object designated, and in most cases there was time to trace it only once.

The table shows no unanimous tendency on single exposures. On simultaneous exposures with alternate tracing the images

TABLE VI
MOTOR DISTRACTION

Subject	SINGLE EXPOSURE				SIMULTANEOUS EXPOSURE						SUCCESSIVE EXPOSURE		
	TIME				REPORT		TIME				REPORT	TIME	
	AROUSAL		HOLDING				AROUSAL			HOLDING		HOLDING	
					Images—alternate								
	Percentage of superiority normal	Difference / P. E.	Percentage of superiority normal	Difference / P. E.	Writing	Normal	Percentage of superiority normal	Difference / P. E.	Percentage of superiority normal	Difference / P. E.	Write / normal	Percentage of superiority normal	Difference / P. E.
Bz	7.0	1.4	.7	.7	55%	0%	3.4	4.5	46.6	6.3	.51	—41.3	9.9
Bz											.18*	—17.0	2.1
D	—6.0	1.2	—4.6	.6	100%	0%	—3.3	.3	66.0	5.8	.30*	5.2	.8
H											1.0	166.0	15.5
K											.92	60.5	6.9
Ms											.24	84.0	13.4
R	37.0	3.2	—26.0	2.9	0%	0%	—15.7	1.3	—42.1	5.3	1.23	61.7	9.1
R											.75*	9.9	1.7
T											.97	-111.0	28.0
L	—12.7	3.1	8.0	1.7	58%	0%	—10.5	1.4	—5.2	.9			
Average........	6.1	2.2	—5.5	1.5	53%	0%	—6.5	1.8	16.3	4.6	.68	24.2	9.7

* Check series; performed later.

alternate in 53% of the trials whereas normally they never alternate. They rise more slowly and hold longer under the former condition, but it is doubtful as to what is thus indicated,—reinforcement or distraction. In successive exposures with motor distraction on the second object it predominates only .68 as frequently as on the normal series and the images hold 24% longer on the normal series.

On single exposures *Bz* frequently writes automatically and keeps the attention on the figure. There is also a " tendency to name some of the figures when writing a hard word,—motor reinforcement to make it persist, and after the flash the vocal cords say ' semi-circle ' and there is more effort put forth to get the image under these conditions." *D* states that the " writing takes away from the clearness of the image; it takes away the attention." *L* finds the " spelling automatic," and speaks of the attention as " divided between work and figure." *R* also finds that " the writing is automatic and does not distract at all even if there is a visual image of the word and the hand." " It is more pleasant to write than not to; it relieves the monotony of fixation."

On the simultaneous series with alternate tracing *Bz* is conscious of eye movements. " I keep the eyes in the direction of the one last looked at; I think the attention goes with the eye muscles; effort to keep the eyes still when the flash comes." *D* notes a " feeling as if the eyes were turning." *L* gets the image in " nearly the same distribution as the object." It may be noted that in the trials in which the images alternate, with *Bz* and *L* the alternation starts with the figure last looked at, while with *D* it starts with the other, i. e., the figure presented first in the tachistoscope. There is one exception in the case of each subject.

On successive exposures *Bz* reports, " If I write carefully it has a distracting effect." Sometimes he keeps " repeating the word during the image." *D* shows two tendencies. The table shows that the writing detracts from the second object in about 70% of the trials. But in the remainder the second object is relatively much clearer than in the normal series. *D* states that the " words sometimes take away from the first although they on the whole detract from the second." He can write automatically. Further, " Normally I get an idea of the first kinaesthetically while looking at the second, and when writing I ignore this." Evidently the writing sometimes operates as a distraction from the second object, and sometimes distracts from the kinaesthetic persistence of the first during the exposure of the second. *K* shows very slight

influence of the writing on the whole. He states, "Tracing the word seems to wipe out the kinaesthetic aspect of the first,—my whole kinaesthetic set as a real object; the movement may interfere with the kinaesthesis that normally develops; tracing seems to wipe out the perseveration of the first." Again, "Tracing does not seem to interfere with the image if I name the first object." Presumably these two tendencies balance one another in the results indicated in the table.

G. *Mental Distraction*

The results of the series with mental distraction are summarized in Table VII. The distraction was produced by performing continuous addition aloud, starting with one number and adding a second to it and continuing to add the second to the resulting sum. On the single exposures and on the successive with distraction on the first figure, the numbers were given about 2 seconds before the appearance of the figure,—instructions such as "17 add 24." On successive exposures with distraction on the second figure, the first number was given before the trial and the other number 1 second before the appearance of the second figure.[22] On the simultaneous method as on the series with motor distraction it was not feasible to test the variable directly. The subjects fixated the figures alternately making four alternations per exposure as directed by the experimenter's signal, "Right, left," etc.

The table shows that on single exposures without distraction the images hold 17% longer than when addition is done during fixation. On simultaneous exposures with alternate fixation the images alternate in 45% of the trials whereas they never do normally. When the distraction is given on the second figure it predominates about .82 as frequently as normally and when the distraction is given on the first figure the second predominates .15 more frequently than normally. The only large differences, however, are with *Bz* and *Ms*. With distraction on the first or second figure the images hold somewhat less than on the normal series.

On the single exposure series *Bz* finds the addition a "pronounced distraction." Consequently he finds that "when the adding is hard I want to shut my eyes for fear of making a mistake." He has been a school teacher and feels badly if a mistake is made. *D* simply says that the "addition takes

[22] This was to enable the subject to start promptly on the addition although not starting during the first exposure as might be the case if the numbers were both given at the outset.

TABLE VII

MENTAL DISTRACTION

Subject	SINGLE EXPOSURE		SIMULTANEOUS EXPOSURE		SUCCESSIVE EXPOSURE					
	TIME		REPORT		REPORT		TIME			
	HOLDING						HOLDING		HOLDING	
	Percentage of superiority normal	Difference / P. E.	Images—alternate: Alternate fixation	Images—alternate: Normal	Add on second / normal	Add on first / normal	Percentage of inferiority add on second	Difference / P. E.	Percentage of inferiority add on first	Difference / P.E.
Bz	31.0	3.0	25%	0%	.29*		—9.8	2.9	—86.0	22.3
Bz					.42	1.27	17.5	2.8		
D	21.0	2.9	87%	0%						
F					.90	1.04	—29.8	9.6	15.8	5.3
G					1.02	1.23	28.8	8.0	70.3	13.8
H					1.00	1.00	125.0	24.0	34.4	13.0
K					.98	1.08	17.6	2.5	53.0	6.0
L	18.0	3.4	66%	0%	1.03	1.01	12.0	3.1	33.3	8.5
Ms					.59	1.45	25.0	6.8	53.0	11.3
R	—1.3	.6	0%	0%	1.08	1.24	62.3	9.2	44.0	7.1
R					.84*		—17.0	2.1		
T					.92	1.00	—206.0	24.8	—157.0	19.7
Average...	17.2	2.5	45%	0%	.82	1.15	4.1	8.6	6.7	11.8

* Check series; performed later.

the attention from the imagery and makes the image less clear." *L* finds the arithmetic "a great distraction, more so than the writing." Occasionally when distracted he "gives it a good look at the end." *R* says, "If I put the attention on the adding it reduces the imagery."

On simultaneous exposures the reports are almost identical with those on motor distraction (supra). *Bz* and *D* note eye movements with the alternating images, while *L* notes the distribution of attention in the image almost identical with that in the stimulus. With *Bz* and *L* the alternation starts with the figure last looked at, and with *D* it starts with the figure that was exposed first.

On successive exposures the introspection indicates that the adding in general affords a distraction. The noticeable thing, however, in Table VII is the marked results for *Bz* and *Ms* and the slight difference for the other subjects. This result might be due to one of several causes: (1) individual differences in the actual effect of attention upon imagery, i. e., whether a conscious visual impression is necessary for the arousal of the image; (2) individual differences in the method of addition,—e. g., whether visualizing the numbers interferes with the visual impression; (3) differences in the degree of attention devoted to the addition,—whether it always occupies the focus and whether the attention fluctuates.

The first of these possibilities would seem to be ruled out by the fact that both *Bz* and *Ms* report trials in which the image appears without there having been a conscious impression of the stimulus, and two other subjects do likewise. As to the second, in a series of tests upon ability in continuous addition under visual distraction *Bz* and *Ms* show a slight loss in efficiency as compared with their normal rate, but four other subjects show the same tendency. Moreover *Bz* visualizes the numbers and *Ms* does not. The third alternative remains and a survey of the introspection tends to substantiate it. Most of the subjects give occasional reports that would indicate fluctuations of attention during the addition, or addition performed with varying degrees of attention. The two crucial subjects do not show these fluctuations. *Bz* adds with a high degree of attention. He "likes to get up to 100." He hates to make a mistake in addition. It is to be noted that in the previous series on single exposures he shows the effect of the distraction the most markedly of the subjects. *Ms* finds that "when the addition went well it attached a pleasant effect to the object and when it went poorly it took the attention from the object." As a matter of fact it seldom

went well and his general opinion at the end of the series was that he "hated it." It is probable that with these two subjects the mental state while observing the object and doing mental addition approaches more nearly and constantly to complete distraction from the object.

Thus the fact of the influence of attention upon imagery which has been more or less manifest throughout the entire investigation receives definite corroboration. Although a complete control of attention is impossible, the results give rather strong indications of the importance, for imagery, of attention bestowed upon the stimulus.

IV. Summary

A. *General Tendencies*

The preceding experiments have investigated the rôle in imagery of complexity of contour, size, length of exposure, interest, motor reinforcement, motor and mental distraction. Each of these variables was applied to a stimulus object and the effect upon subsequent imagery noted. Three methods were used in each case,—single, simultaneous and successive exposures,—with fairly consistent results. The first five variables mentioned tend quite generally to reinforce visual imagery and facilitate its arousal, and the last two have the opposite effect. If the general averages at the bottom of the columns in Tables I-VII are considered it is evident that the smallest differences are shown by all three methods for size and length of exposure. Motor reinforcement yields a marked effect by all methods, and interest also ranks quite high. The effect of complexity and motor distraction is considerable in simultaneous and successive, and that of mental distraction in single and simultaneous exposures.

B. *Individual Differences*

Average results must not be allowed to obscure the individual differences, for while the subjects nearly always show the influence of the factors in question, there are great differences in degree. Furthermore the introspection reveals that the given factors operate by different means with the different individuals.

If the results for the subjects who participated in the experiment on a number of variables are followed through Tables I-VII, it is evident that individuals vary as to the factor that most affects them. For instance, *Bz* seems most affected by motor reinforcement and least by size. *D* is most

susceptible to motor reinforcement and least to lengthening of exposure *L's* imagery is most facilitated by interest and least by lengthening of exposure. *R* is most influenced by complexity of contour and least by distraction.

In the course of the work a number of individual tendencies have been brought out. The salient points for each subject may be briefly mentioned.

Br notes the presence of kinaesthetic factors during the presentation of the stimulus, which apparently facilitates the subsequent imagery.

Bz works kinaesthetic imagery to aid the visual and in addition to central kinaesthesis actual eye movements are frequently noted. The distribution of attention during the stimulus is a great factor in influencing imagery. If one part is fixated that part becomes clearer in imagery.

C has a tendency to accentuate attributes of the stimulus such as size.

D on successive exposures has a tendency toward a persistence of the first object in kinaesthetic terms during the exposure of the second. Any distraction during the second or a shortening of its exposure tends to interfere with the persistence of the first and to detract from its subsequent imagery. Much depends too upon distribution of attention during the stimulus period.

F shows considerable play of associations with simple figures and this correlates in general with reproductivity of the figures in imagery. It is the pleasant associations that have the greatest effect.

H's most marked feature is a pronounced visual perseveration. In single and simultaneous exposures the images usually appear very quickly and exactly reproducing the stimulus object. In successive exposures the second object appears alone in imagery under all conditions, and rises quickly.

K shows like *D* a kinaesthetic persistence of the first object during the second on successive exposures. Distraction on the second militates against imagery of the first.

L reports far more kinaesthesis than any of the other subjects. The visual image is frequently filled in with kinaesthesis. In the voluntary arousal of a visual image with merely a word stimulus, he experiences great difficulty in getting a visual image at all. Kinaesthetic imagery is sometimes used to mediate the visual. On successive exposures there is frequently a motor memory of the first object during the second. The distribution of attention in the imagery follows that in the stimulus. In some cases an attribute of an

object appears in imagery although the object itself does not do so.

Ms has a great play of associations. The objects tend to become real in imagery with a very definite and complex context. This tendency correlates with reproductivity to a considerable extent. He also occasionally notes kinaesthetic imagery in conjunction with the visual.

Mt has kinaesthesis frequently in connection with the visual imagery. She feels it would be easier to draw the figure than visualize it, and there is a tendency to draw the part that is interesting.

R, after tracing an object, often recalls it for visual imagery by an actual movement of the hand. In many other instances he names the object as an aid in recalling it.

C. *Conclusions*

1. Complexity of contour of a plane figure, increase of its size, lengthening of its exposure, interest aroused in it and motor reinforcement by tracing its outlines during fixation tend, in general, to facilitate the arousal of visual imagery of that figure. Distraction by performing mental arithmetic or by writing extraneous words during fixation has the opposite effect. The results are, on the whole, most marked with motor reinforcement and with interest, and least marked with increase of size and lengthening of exposure.

2. Individuals differ in their susceptibility to the above factors. The visual imagery of some is most influenced by motor reinforcement; of others by interest; and of others by complexity of contour.

3. Individuals differ further in the means by which these factors operate. The principal points of difference are:

A. Other imagery,—notably kinaesthetic,—is with some individuals involuntarily employed to reinforce or mediate the visual. There may be kinaesthetic imagery of the movement of the hand or eyes around the contour of the object, or verbal imagery of its name.

B. Some individuals have a kinaesthetic persistence of a visual stimulus immediately after its disappearance, and interference with this detracts from subsequent visual imagery of the object.

C. Distribution of attention during the stimulus period conditions with some individuals the distribution of attention during the subsequent imagery.

4. It seems probable that the factors studied in the present experiment are somewhat interrelated and converge toward

a single factor,—the directing of attention to the stimulus object. The introspection indicates that the effect of complexity of contour and the effect of size are due primarily to motor elements. The motor aspect, in turn, is described as producing a greater concentration of attention. Interesting material is reported as arousing associations and stimulating attention to a greater degree. When the attention factor is made the subject of specific study by means of distraction during fixation of visual stimuli, the results show that, although in certain sporadic cases the arousal of imagery of the stimulus may proceed entirely involuntarily, a higher degree of attention devoted to an object generally facilitates the arousal of imagery of that object.

5. Apart from the question of "visual type," there are individuals who can reproduce a visual image immediately, and others for whom the visual is mediated or reinforced by imagery (or in some cases sensation) of another mode,—notably kinaesthetic. Grouping of individuals on the basis of such a tendency shows no correlation with a grouping on the basis of clearness of persistence of imagery.

PROFESSOR MÜNSTERBERG'S VOCATIONAL TESTS[1]

By Harold E. Burtt

I. Introduction

During the spring of 1916 Professor Münsterberg gave a series of mental tests to the members of his large course in elementary psychology. The work was undertaken primarily because of interests in vocational psychology and an attempt was made to correlate the results not only with collegiate standing but with individual estimates of vocational aptitude. The same tests were subsequently given, in whole or in part, to a small class of Radcliffe students, to 31 members of a western military academy, to 40 employees of an Ohio clothing house, to 23 executives and salesmen of a large New England manufacturing concern and to 72 employees of one of the leading Boston department stores. In some cases employers' estimates of the vocational ability of the persons tested were available.

At the time of Professor Münsterberg's death the original data from the 460 subjects had been corrected and scored and certain correlations computed for the Harvard group.[2] The vocational aspect of the tests and the interpretation of results for other groups than the Harvard students was barely begun. At the same time more detailed materials for some of the tests were being worked out and various forms of new tests were being devised with a view to future standardization.

The entire data was turned over to the writer by Mrs. Münsterberg with a view to posthumous publication. The

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[1] From the Harvard Psychological Laboratory.

[2] A popular article based on this aspect of the tests was written by Professor Münsterberg for the *Harvard Illustrated*, **18**, 170.

writer was in close touch with Professor Münsterberg and fairly familiar with his methods and purposes and, while it is impossible to do the matter justice, an attempt will be made to compute and interpret the results as Professor Münsterberg intended to do.[3]

The following account will comprise six parts: (1) a brief description of the tests employed; (2) norms for the various groups tested; (3) correlations for the group of Harvard students of test scores with class standing and the intercorrelations of some of the tests; (4) the differential vocational sensitivity of the tests on the basis of the extremes of personal estimates as to vocational aptitude in the Harvard group; (5) the vocational aspects of the tests indicated by the results from industrial concerns; (6) suggestions as to other new forms of tests and general vocational possibilities.

II. Description of the Tests

The test material was presented to the subjects in groups by means of printed blanks.[4] The thirteen tests used will be designated by letters of the alphabet.

Test A. *Free association.* Stimulus word (e. g. railroad, poetry) spoken by the experimenter. Subject writes a train of association words for periods of 1, 3 and 5 minutes in successive trials with different stimuli.

Test B. *Attention (intellectual).* Three hundred and fifty unspaced capital letters arranged in several lines, certain adjacent groups of letters forming a word. Subject underlines such words. For example in "hotbrdenexerfisaelieyur" are the words "hot, den, is, lie." There are 55 such words in the list. Time 3 minutes.

Test C. *Attention (perceptual).* Similar to the above, having 350 unspaced digits. Subject underlines adjacent pairs whose sum is 9. Thus in 65408618534927 are the pairs 54, 18, 27 whose sum is 9. Time 3 minutes.

Test D. *Construction by selection.* A list of several words such that by selecting one letter from each word the name of some famous American can be spelled. Thus: (1) *b*all, tu*r*n, e*y*e, l*a*rd, k*n*ife,—Bryan; (2) b*e*ar, roa*d*, p*i*ne, ro*s*e, h*o*le, tow*n*,—Edison. The italics are not indicated on the test blank. Five lists of this sort constitute the test. Time 4 minutes.

Test E. *Completion (intellectual).* Two hard examples for

[3] The writer is indebted to Professor Münsterberg's secretary, Miss Wilkins, and to Professor Yerkes.

[4] The form of all the tests is such that they can be reproduced on a typewriter without the use of special drawings or charts.

sentence completion, giving the first letter of each omitted word and indicating by asterisks the number of letters necessary to complete the word. Seven and nine words omitted respectively in the two selections. Time 2 minutes each.

Test F. *Completion (perceptual)*. A list of words having only the first and last letters given; thus: p-l, a-y, w-h. Subject fills in any number of letters to make the word. In a second list the first two letters of the words are given; thus: cu-, tu-, ai-, and the words are to be finished. Thirty words in each list. Time 2 minutes each.

Test G. *Word building* with the letters a, b, e, l, r. Time 3 minutes.

Test H. *Decision.*[5] A group of 4 lines of vowels such as the following:

E U U O O U U E A A E U U E A A E U U O O U U E
A U A O U U U O U A U O O U A U O U U U O A U A
E O U O U O A U U A U U U U A U U A O U O U O E
U E E U O A U E E A E U U E A E E U A O U E E U

In the above group U occurs 42 times and the other vowels 18 times each. Seven other groups on the blank have the following ratios: O 42, others 18; O 30, others 22; E 42, others 18; U 30, others 22; A 30, others 22; A 42, others 18; E 30, others 22. Subject looks at the sheet and judges (without counting) in each group as to the predominant letter. Time 2 minutes.

Test J. *Reasoning (relation)*. Twenty adjectives such as "busy, clumsy, cruel," printed in a column. Subject writes after each its opposite. Time 2 minutes.

Test K. *Reasoning (principle)*.[6] Three rows of vowels A, O and U such as the following:

A U O A A
A O U
A U U A O A A A U

The O bears a constant relation to the other vowels in each line (in this case the middle). Six similar groups embodying the following relations: fifth from the right end; before the first U (counting from the left); second after the first U; second after the second A; first before the third A; as many places after the first A as this is after the beginning. Six minutes to discover and write down the various relations.

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[5] Described originally in Münsterberg, H. *Psychology and Industrial Efficiency*. 1913. p. 86.

[6] This test was suggested by the Yerkes multiple choice method. *cf.* Yerkes, Robert M. The study of human behavior. *Science*, 39, 625.

Test L. *Reconstruction* (*perceptual*). Nouns with the letters disarranged. Subject discovers the word represented. Thus: "ettrul," "idolcocer" are "turtle" and "crocodile." Eight names of animals and eight names of cities in the United States. Time 3 minutes each.

Test M. *Reconstruction* (*intellectual*). A sentence with the first few words in the correct order but with the remainder arranged alphabetically, such as the following:
"The idea of mental tests is—abilities acquired and and compare discover demands inborn is mind of of practical the the the the them to to various vocations with." Two other selections of about the same length and difficulty. Total time for rearrangement of the three, 9 minutes.

Test N. *Sentence building* (used in place of test M with all but the Harvard group). Ten nouns and adjectives such as, "book, butterfly, green, house, picture, milk, serious, seven, smile, woman." Subject forms a sentence containing all these words. Two groups of this sort. Time 4 minutes each.

All the data except that for test N was corrected quantitatively and the results then transformed according to an arbitrary scale which varied for the different tests so that the final scores formed roughly a probability distribution with a minimum of 0 and a maximum of 10 points. For example in test B the correct underlining of 1-20 words gave a score of 0 points, 21-25 words 2 points, 26-30 words 4 points, 31-35 words 6 points, 36-39 words 8 points, 40 words or more 10 points. In this way a single digit from 1 to 10 represented the individual's performance in each of the 12 tests.

The tests occupied approximately two hours and in all cases were given in two sets on different days. About five per cent of the Harvard group were absent on each day and were given a chance to make up the work. None of the blanks were allowed outside the room, and as the total average score of those taking "make-ups" differed from the general average by less than two per cent, no appreciable error was introduced by this procedure.

In all the groups but two the tests were given by Professor Münsterberg. For the students in the military academy and the employees of the Ohio firm full directions were forwarded and a responsible person gave the tests.

III. General Averages

The average scores in the separate tests for each group of subjects are given in table I. The successive rows indicate the 13 tests and the columns indicate the groups. At the head of

each column is given the number of individuals in the group. At the bottom are the sums of the scores for tests B to L inclusive. A was scored by a slightly different method in some of the groups and the results are not strictly comparable.

A glance at the bottom row of the table shows that the rank of the five groups in the sum of the test scores is what might be expected on the basis of the selection of the groups. The Radcliffe students in an advanced course in psychology rank highest. The Harvard group ranks next as might be expected on the basis of selection. Then follow the military students in a secondary school who presumably were not as thoroughly selected by entrance requirements; the salesmen and executives of the wholesale concern, and finally the unselected clerks in a department store.

TABLE I

Test	Harvard (276)	Radcliffe (17)	Military academy (31)	Wholesale employees (23)	Boston dept. store (72)	Ohio dept. store (40)
A. Association.......	4.9	8.0	7.4	4.3	5.4	...
B. Attention (words).	5.4	7.0	4.1	5.1	5.5	...
C. Attention (letters).	4.4	7.9	7.0	4.8	4.1	...
D. Const. by selection	4.8	3.3	3.2	3.9	1.7	...
E. Sentence compl't'n	4.4	5.4	2.7	2.5	1.1	2.7
F. Word completion.	5.1	7.4	5.8	4.9	4.9	3.0
G. Word building....	3.3	4.9	2.9	2.3	1.5	2.1
H. Decision.........	4.2	4.7	4.4	3.9	3.5	...
J. Opposites........	6.7	8.8	3.5	4.5	2.5	...
K. Principle.........	4.5	2.6	2.5	2.5	1.6	...
L. Rearrange letters.	3.9	3.8	3.0	2.7	1.2	2.9
M. Rearrange words..	5.2	...	...	...	...	...
N. Sentence building.	...	6.0	6.4	5.0	3.6	...
Sum B-L............	46.7	55.8	39.1	37.1	27.6	...

If in the table we regard the Harvard group (which is by far the largest) as the standard and compare the others with it in respect to the various tests we note that the Radcliffe group is considerably superior (80%) in test C and considerably inferior (42%) in test K. The military academy group also excels the Harvard in C (59%) and falls farthest below it in J (48%) and K (45%). The wholesale employees fall farthest below the Harvard averages in K (45%) and E (44%), and the clerks are most inferior in E (75%) and L (69%). The most noticeable differences, then, are in test E, sentence completion,—frequently mentioned in the literature as a test of intelligence,—and in tests J and K which involve reasoning and the ability to perceive relations.

It is possible to examine the data from another standpoint, namely the ranking of the average scores for the ten tests within a given group of subjects. Thus test B yields nearly the highest score of the ten in every group. So does C, with the exception of the Harvard students. E stands relatively high in rank with both college groups, but relatively low with the other three groups. H ranks rather low with the college groups and relatively high with the others. J ranks the highest of the ten for the college groups and fourth or fifth with the others. K ranks very low for all the groups except the Harvard, where it ranks fifth. Here again the most striking thing is the superiority of the college students in tests E and J, sentence completion and opposites, tests of a distinctly intellectual order.

The ranking of the tests in the various groups do not intercorrelate highly except in a few instances. The ranks of the ten tests for the wholesale group and for the department store employees correlate to the extent of $.70 \pm .14$ by the foot-rule formula. The military group correlates with each of these to the extent of .62 and $.64 \pm .14$. All other correlations are less than .30.

IV. Correlations

Various correlations were computed for the Harvard group. It seemed interesting first of all to compare efficiency in this set of tests with class standing. Inasmuch as the group was composed of members of the four undergraduate classes the marks for the freshman year were taken as a standard. Arbitrary values were assigned to the marks of A, B, C, etc., and an average coefficient determined which represented the individual's college standing for the year. These coefficients were then compared with the sum of the test scores for the different

individuals. If the 276 members are distributed in tertiles of rank in both respects the results are as shown in table II.

TABLE II

CORRELATION OF CLASS STANDING WITH SUM OF 12 TESTS

	1st tertile tests	2nd tertile tests	3rd tertile tests
1st tertile class..............	43%	35%	22%
2nd tertile class..............	33%	28%	39%
3rd tertile class..............	24%	37%	39%

There is evidence of some correlation but it is not very striking. If the ranks are correlated by the foot-rule method the coefficient is $.127 \pm .026$. If class standing is correlated with the sum of the tests E and M (which perhaps lay more stress on intellectual processes) the coefficient is somewhat higher ($.178 \pm .026$). This rather low correlation coincides with the results of many similar studies. It suggests the fact that many students (in freshman year at least) do not work at their maximum level of intellectual efficiency.

Certain of the tests were correlated with one another, with class standing and with the sum of the tests by a rough use of the foot-rule formula. The only point of interest is that the sum of the tests correlated most highly with tests B and C,—the two attention tests. This suggests the fact that in these two tests the initial adaptation to the conditions of the test is slight and they are thus more reliable than other single tests in hasty application.

V. VOCATIONAL IMPLICATIONS OF THE TESTS IN THE HARVARD GROUPS

The Harvard students who took the tests were asked to give an estimate of their own ability as executives, salesmen and promoters or inventors. Each man was to give himself a total of 100% in the three fields. Then groups were formed on the basis of extremes of such estimates for a comparative study of the vocational sensitivity of the tests. Thus a group was formed of approximately 20 students who estimated their ability as executives at upwards of 80% and such a group contrasted with another formed of about 20 who estimated their executive ability at 10% or less. Similar groups of about

the same numbers were formed for salesmen and inventors. Individual estimates of this sort would undoubtedly be unreliable in the middle range, but it is probably safe in a preliminary survey, to attach some validity to the extremes of personal estimates.

The average scores of each of these six groups for each of the twelve tests are given in table III. The successive columns give the averages for the 12 tests A to M. The first row gives the average in those tests for the group of good salesmen, the

TABLE III

Test		A	B	C	D	E	F	G	H	J	K	L	M
Salesmen	Good	5.7	6.1	4.8	4.9	4.0	6.0	3.6	3.6	6.8	4.8	5.1	5.2
	Poor	4.8	4.9	5.5	5.3	5.7	5.5	3.2	5.1	6.3	4.4	3.2	4.5
	% superiority good	19	24	–13	–8	–30	9	12	–29	8	9	**59**	15
Executives	Good	5.0	5.0	4.7	4.5	5.2	4.7	3.1	4.2	6.9	3.7	3.5	5.4
	Poor	4.2	4.3	4.3	2.8	3.2	5.1	3.1	3.2	6.3	5.2	2.9	5.2
	% superiority good	19	16	9	**61**	**62**	–8	0	**31**	9	–29	21	4
Inventors	Good	4.0	4.5	5.2	3.3	4.4	4.8	2.7	4.4	6.4	5.2	2.2	5.2
	Poor	4.1	5.3	5.8	3.7	3.5	4.8	3.1	3.7	6.2	3.6	3.4	5.3
	% superiority good	–2	–15	–10	–11	26	0	–13	19	3	**44**	–35	–2

second row for the group of poor salesmen and the third row the per cent by which the figure in the first row exceeds the corresponding one in the second, i. e. the per cent by which the good salesmen are superior to the poor in the various tests. Similar figures are given in subsequent rows for executives and inventors.

A glance at the rows of per cents difference shows the tests which in the above group are most sensitive to the three vocational aptitudes. In salesmanship the most marked results are with test L (rearrangement of letters). In this test the good salesmen are 59% superior to the poor and the difference is 3.2 the probable error of difference. The next greatest difference is in test B (attention-intellectual) with a difference of 24% which is only 1.8 the probable error of difference. The superiority of the good salesmen in the other tests is slight or negative.

With executives there appear three fairly sensitive tests,—D (construction by selection) and E (sentence completion),

with differences of 61% and 62%, almost equally sensitive, and H (decision), with a difference of 31%, sensitive to a somewhat lesser degree. The three differences are respectively 3.6, 3.3, and 2.0 the probable error of difference.

Only one test appears at all sensitive to inventive or promotor ability, viz.: K (principle). The difference is 44% and is 3.4 the probable error of difference.

Thus, assuming a certain validity in the methods of selecting the above groups, there are one or more tests which appear somewhat sensitive to each of the three vocations in question. A further problem is the comparison of these figures with results of the application of the same tests to efficient and inefficient salesmen and executives in actual practice.

VI. Vocational Implications of the Tests in the Industrial Groups

The employees of a New England wholesale concern who were given the tests were also rated by their managers with reference to ability as salesmen or executives. It was thus possible to select efficient and inefficient groups of each sort,—the only difficulty being the small size of the groups.

Thus comparing the test results of the 5 best salesmen with those of the 5 worst and computing the data as in table III there appear possibly significant differences in the following tests:

Salesmen

Test	C	D	F	J	L
Good	7.5	5.5	7.5	5.5	3.5
Poor	4.0	2.4	4.0	2.8	1.6
% superiority good	87	130	87	96	120

The good salesmen actually score more highly in most of the tests but the greatest differences are manifest with D (construction by selection) and L (rearrangement of letters). The latter is the test which appears most clearly differential of salesmanship ability in the Harvard results.

Similarly comparing 4 good executives with 3 poor ones: Here although the good executives are superior in most of the tests, the greatest difference is for E (sentence completion) which yields the greatest difference also in the Harvard group.

Four of the tests. F F, G and I were given to 40 employees

EXECUTIVES

Test	E	G	K	L
Good	6.0	5.5	4.5	5.0
Poor	1.3	2.0	1.3	2.0
% superiority good	360	175	246	150

in an Ohio department store. A person familiar with the individuals selected 10 good salespeople and 10 poorer, also 10 good executives and 10 poorer. The results for the salespeople are negative. In none of the tests are the good salespeople appreciably superior on the average. This is possibly due to the small size of the group and its selection. The groups of salespeople were extremely variable, having a mean variation of over 100% in several instances. With the executives the results are clearer as indicated by the following:

EXECUTIVES

Test	E	F	G	L
Good	4.8	3.4	2.4	3.8
Poor	2.2	2.4	1.2	2.0
% superiority good	118	42	100	90

The good executives are superior to the poor in every test but especially so in E (sentence completion). It is to be noted that E gives the most distinct results in the two studies just mentioned.

The salespeople in the Boston department store who took the tests were selected by the superintendent so as to give a group of 35 efficient and one of 35 inefficient clerks. The tests which yield the greatest difference in the average scores of the two groups are as follows:

SALESPEOPLE

Test	D	E	F	L
Good	1.9	1.4	5.5	1.5
Poor	1.4	.9	4.2	1.0
%superiority good	36	55	31	50

Test E (sentence completion) and L (rearrangement of letters) show the greatest sensitivity to salesmanship ability in this group. The latter shows similar sensitivity in the other studies just mentioned.

These results may be, of course, somewhat invalidated by the small number of individuals in some of the industrial groups and by the use of personal estimates of ability in the Harvard group, but it is a significant fact that certain of the tests are found to be consistently differential of vocational aptitude in all the various groups investigated. Test E (sentence completion) proves to be in the above study a test par excellence of executive ability, and test L (rearrangement of letters) of ability in salesmanship. The results manifestly warrant further investigation.

VII. Suggestions for the Future

In addition to the above, a number of new forms of tests were being devised by Professor Münsterberg with a view to further vocational study and standardization.

1. Test of observation. A series of numbers arranged thus:

12	3	14	9	5
7	15	17	2	21
22	1	8	11	18
24	6	13	23	16
20	25	10	4	19

involving, however, numbers from 1 to 100 instead of merely numbers up to 25. The subject marks numbers 1, 2, 3, etc., in order up to 100 with a time limit.

2. Test of foresight. A labyrinth made of letters x and o, thus:

Z	o	o	o	x
o	x	x	o	o
x	o	o	o	o
x	o	o	x	x
o	o	o	o	A

but much more complicated and involving 224 letters. The subject starts at A and by tracing on the o's reaches the opposite corner Z.

3. Test of suggestibility. Two columns of words at opposite sides of the sheet, thus:

	tree
	bird
	cat
dog	ink
	blue
	grass
shirt	boat
	collar
	eye
	library
book	loud
	truth

The subject has to quickly indicate which words in the second column are on the same line as the various words in the first column. For example " ink " is on the same line with " dog," but the word " cat," a line above, is more strongly associated with " dog " and may act as a suggestion and appear on the same line. Similarly " collar " is a line below " shirt " but more strongly associated with it than " boat." Long lists of this sort may give an index of suggestibility.

4. Test of " steadiness." A selection beginning as follows: phree stchraute blyoovcextbhl rhyiusi elliffmee tsrheaclslt bluoysree pirte. By reading the alternate letters of each word beginning with the second, one may decipher the sentence, " He that loveth his life shall lose it." In other material every third or fourth or fifth letter is to be read. Thus: obnvttrweovniugxcuiecvbitpuythercuexciurswe spells " together " by reading every fifth letter. The ability to hold a given setting and rapidly decipher a fairly long selection is an index of " steadiness."

5. Test of recognition. Two stories differing only in minor details. The subject reads them successively and in the second indicates the points that have been changed.

6. Test of division of attention. A selection in which alternate words beginning with the first form one story and alternate words beginning with the second form a second story, thus: " I The want little to house consult where you, I Professor was Brown, born, about and a in most which baffling I case passed of the sudden earlier death years under of suspicious my circumstances. life." The subject reads the selection intact without pause and then reproduces the two stories as far as possible.

The ultimate intention of Professor Münsterberg was to arrange two sets of tests,—one for vocational guidance and one for vocational selection. In the former case the individual

was to be provided with a set of blanks for each of the tests used. An elaborate and detailed set of examples for the given test was to be given on the first sheet and the subject was then to do the tests on the remaining sheets after having the method made clear by the examples. Materials arranged in this way were to be provided for each test. Then by comparing his results with vocational norms for the given tests the individual could determine his aptitude. A somewhat similar set was to be provided for employers interested in vocational selection, although the two sets would not be identical. Tests involving suggestibility, for example, would not be feasible for auto-experimentation.

In providing these sets the tests which proved in the above and in other studies to be the most sensitive to vocational aptitude were to be standardized with more exhaustive material and vocational norms determined. It would seem that of the twelve tests used in the above investigation the most profitable ones to standardize would be tests of rearrangement of letters for salesmen and completion tests for executives.

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THE PERCEPTION OF SLIGHT CHANGES OF EQUILIBRIUM, WITH ESPECIAL REFERENCE TO PROBLEMS OF AVIATION[1]

By HAROLD E. BURTT

I. *Introduction*

Previous experiments upon the perception of changes of equilibrium have been performed on the tilting-board.[2] The subject, usually prone upon the board, was tilted to various positions and required to report the angle of tilt or to indicate the vertical. It is obvious that this type of estimation is quite dissimilar to that made by an aviator during flight. In an airplane one occupies a sitting posture and responds to relatively slight deviations from the perpendicular, and it is a case not of deliberate judgment but of choice reaction, correcting the motion of the plane the instant the direction of the tilt is perceived. Accordingly some experiments were performed bearing more directly upon the practical situation. Two problems were studied:

1. What is the relative delicacy of perception of slight changes of equilibrium in the various directions right, left, front and back? Differences in such perception, if general, would have a practical bearing on the selection or arrangement of airplane controls so as to facilitate reactions to the more difficult perception.

2. What are the merits of dep as compared with stick airplane control in reacting to slight changes of equilibrium? If the use of one hand on the stick or two hands on the dep

[1]From the Harvard Psychological Laboratory.

[2]This work is summarized in Nagel's *Handbuch der Physiologie des Menschen*, 1905, vol. 3, p. 736 ff.

and the different movements involved constitute a variable sufficient to influence the reaction time, this fact might warrant the general adoption of one or the other type of control.[3]

II. *Method*

A heavy wooden platform 1 meter square was mounted so as to rock about a point 35 cm. below its centre. Iron braces from its corners met at a rounded plate below the centre and this rested on a flat plate on the base of the apparatus. A loose bolt held it in place. Heavy spiral springs under a little tension were attached from each corner of the platform to the base directly below. The platform was thus held normally in a horizontal position, but a moderate force applied to a corner would tip it in the desired direction.

The platform was tilted in some of the experiments by means of a crank and worm gear mounted on an upright about 50 cm. back of the platform. A sprocket on the shaft with the worm carried a short chain from the ends of which wire ropes led to two opposite corners of the platform through pulleys directly below the sprocket and other pulleys below the corners. The ropes were made taut by means of turnbuckles. Turning the crank lowered a corner of the platform and turning it in the opposite direction righted the platform or lowered the opposite corner. The wire ropes could be replaced at will by a set connected similarly with the other two corners. The arrangement was such that one turn of the crank lowered the desired corner about 6.35 mm., and the speed and distance could thus be controlled.

This arrangement, however, permitted the tilting of the platform at only relatively slow speeds, and therefore in some of the experiments a different system was employed. A heavy spiral spring, (heavier than those at the corners of the platform) was hung on one end of the chain on the above mentioned sprocket. From the lower end of this spring another chain passed under a second sprocket mounted on the base to the wire rope leading to a corner of the platform. Fastened rigidly to this second sprocket was a ratchet held by a pawl. Thus the heavy spring could be placed under any desired tension between the upper sprocket and the ratchet without disturbing the platform. When the ratchet was released the lower sprocket acted as a pulley while the upper one was stationary and the corner of the platform was quickly lowered,

[3]Two untechnical accounts of these experiments by the author have appeared in *Aviation and Aeronautical Engineering*, vol. 2, 1917, pp. 218 ff. and pp. 562 ff.

the rate and distance depending on the amount of tension placed on the heavy spring. An identical arrangement was connected to the other end of the upper chain so that two opposite corners of the platform could be operated reciprocally by turning the crank in the desired direction and thus operating the desired spring and ratchet. A single lever threw both pawls simultaneously and whichever spring was under tension would operate. Trials in two opposite directions could thus be given rapidly. To change to the other two directions required only a few minutes to connect the other ropes to the chains and adjust the turnbuckles. The springs gave the platform a negatively accelerated tilt such as an airplane makes when struck by a puff of wind or the like.

A chair was mounted on the platform facing one corner in such a way that the vertical axis through the center of gravity of the subject sitting in a normal position passed through the center of the platform. Thus a lowering of the corners tipped the subject directly forward, backward, right or left. In the work on the first problem the chair was an ordinary one which placed the subject's ears approximately 160 cm. above the point about which the mechanism rocked. In the second problem a lower chair was used and the subject's feet rested on a bar only a little lower than the seat of the chair (the position usually occupied by an aviator). In this case the subject's ears were about 130 cm. above the rocking plate.

Visual factors were eliminated throughout the entire experiment, the subject always having the eyes closed during a trial. The subjects with one exception were graduate students in the Harvard Psychological Laboratory. The remaining one had had considerable psychological experience. Two of the subjects were women.

III. *Perception of Lateral vs. Longitudinal Changes of Equilibrium*

In studying the delicacy of perception of a tilt in various directions both forms of apparatus mentioned above were employed. With the first arrangement the experimenter turned the crank at a constant rate (1 revolution in 2 or 4 seconds) timing himself by a pilot lamp flashed once a second by the laboratory time circuit. This speed lowered the desired corner of the platform at a constant rate of 3.175 mm. or 1.587 mm. per second.[4] This movement was practiced for several hours prior to the actual experiments and could be made quite

[4]A drop of 3.175 mm. at the corner corresponded to an angle of 20 minutes from the vertical with the rocking plate as a centre.

even and continuous. The movement always started from the horizontal position,—the platform being righted between trials. The subject held in his lap a light piece of wood carrying two telegraph keys so mounted that the first and second fingers of the right hand operated the left and right keys respectively, while the thumb and other fingers grasped the edges of the base.

The subject was instructed to close his eyes at the signal "ready" keeping his head upright and as soon as he was certain of the direction in which he was being tipped to press the corresponding key. The movement began about one second after the signal. The lateral and longitudinal series were performed separately, so the subject had always a choice between two alternatives, right-left or forward-backward. The right key was for the right tilt in one series and for the forward tilt in the other.

An electric contact was made by the crank when the platform started from the normal position and another when the subject pressed his key. The time was recorded by an electric counter in series with a 10vd. tuning fork or by a 50th second stop watch operated magnetically. Thus the longer the time the greater the distance or angle of tilt and the poorer the discrimination of movement. A double throw switch put a telephone receiver in series with the wrong key so that mistakes were heard by the experimenter.

In one hour four short series were given, e.g., lateral, longitudinal, longitudinal, lateral, and the average of one and four compared with that of two and three, thus avoiding undue influence of practice and warming up. During the next hour with that subject this procedure was usually reversed, i.e., in the above example starting with longitudinal. In a given series the two alternatives were presented in a chance order determined by shuffling a pack of cards. There were 10 to 15 reactions in a series, that is 40 to 60 during an hour.

Table I summarizes the results. Each row represents one hour's work. The first column gives the subject. The second column shows which of the four short series was given first, e.g., "Longitudinal" means that the order on that day was longitudinal, lateral, lateral, longitudinal. The third column indicates the percent by which lateral discrimination is superior to longitudinal, i.e., the percent difference between the average of the two lateral series and the average of the two longitudinal in terms of either time or distance. The last column gives this difference divided by the probable error of the difference. In the results presented in the upper half of the table the

TABLE I

DETECTION OF LATERAL VS. LONGITUDINAL MOVEMENTS AT SLOW RATES

Subject	Order	% superiority lateral	Difference / P. E. difference
An.........	Longitudinal.......	— 8	1.2
An.........	Lateral............	65	6.6
Ap.........	Longitud nal........	15	2.2
Ap.........	Lateral............	— 4	0.4
B..........	Longitudinal........	49	5.7
B..........	Lateral............	32	6.0
C..........	Longitudinal.......	33	2.4
C..........	Lateral............	41	2.5
O..........	Longitudinal.......	41	11.2
Sp.........	Longitudinal.......	37	2.7
Sp.........	Lateral............	—13	1.7
An.........	Longitudinal.......	25	2.6
An.........	Lateral............	30	2.9
Ap.........	Lateral............	31	3.5
B..........	Lateral............	14	2.7
C..........	Longitudinal.......	42	3.4
C..........	Lateral............	7	0.8
O..........	Longitudinal.......	27	5.5
O..........	Lateral............	6	1.9
Sp.........	Longitudinal.......	33	4.1
Sp.........	Lateral............	13	1.6
Average........................		24.6	3.4

corner of the platform was lowered at the rate of 1.587 mm. per second and in the remainder at the rate of 3.175 mm. per second.

The general tendency of the results is obvious. Eighteen of the 21 series show a superiority in the detection of lateral movement, with an average superiority (including the negative values) of 25 percent. The differences are on the average 3.4 the probable error of difference. The mean variations (not given in the table) are slightly larger on the longitudinal than on the lateral, averaging 30 percent and 26 percent respectively. There are also more errors on the longitudinal than on the lateral,—17 percent of the total trials vs. 10 percent. The reactions to the lateral movements are obviously superior in all respects.

At the end of each hour's work the subjects were asked for introspection regarding the means by which they discriminated the movements, the comparative ease of the various judgments and anything else they may have observed. Specific questions were avoided in order to obviate undue suggestion

or the introduction of a different set or attitude toward the experiment.

The criteria involved in the perception of movement of this sort appeared to vary considerably with the different subjects. *An* relied mainly on the static senses. "I perceive the movement in the head; but sometimes I feel it all through the body." *Ap* on the other hand appeared more dependent on his tactual sensations. "Get sensations from back, seat or feet." The pressure of the chair against his shoulder was mentioned most frequently. However he noted having the head in a position in which he could feel them,—"a delicate adjustment which it was hard to get." He sometimes noted a "pure bodily movement which was not localized." *B* noted several factors. "Usually feel it up and down back and in the head. Feels like a pulling around the waist. Sometimes feel feet slipping or feel self slipping on the chair." The tactual criteria seemed more marked in the lateral and the "head" criteria in the longitudinal movement. *C* on the longitudinal felt the platform under the feet or the seat. On the lateral he felt "as if a stick right down through the body tipped." Later he localized the motion outside himself. "I feel stationary and the plane tips under me." *O* judged mainly by "tactual sensations from the chair and kinaesthetic sensations in the neck." Static senses did not seem involved. *Sp* noted for the lateral movement, "Feels like a dumbbell through the head and I wait and see which way ear goes. I feel it in the ear." The longitudinal movement was harder to describe although it seemed in the head also. When she relaxed, however, she felt it in the body, especially in the legs. Thus the perception of a change of equilibrium such as the above seems in general to involve both tactual and static senses. One subject seemed to utilize the former solely but the other subjects employed both.

A further point of interest is the comparison of the subject's estimate of his efficiency in the various directions with his actual efficiency. *An* was "more confident of the sideways judgments; the others lacked clearness." This corresponded to his objective results. *Ap* noticed no difference in his ability to make the various estimates. *B*'s introspection was equivocal, some days considering the lateral the easier and some days the longitudinal, with no correlation with objective efficiency. *C* felt consistently more efficient on the lateral as was actually the case. *O* sometimes noted one direction easier and sometimes the other and there was no correspondence with objective results. *Sp* consistently reported the longitudinal as much

more difficult and often unpleasant. However she tried to concentrate more on it and this sometimes reversed her objective results. Thus subjective estimates of ability to detect lateral vs. longitudinal movements do not correlate uniformly with actual ability.

In the results thus far described two rates were used, the interest being in the direction rather than the rate of movement. A few series were performed to test directly the effect of the latter variable. Trials at the two rates of 3.175 and .793 mm. per second lowering of the corner of the platform were intermixed in irregular order. The time of reaction and the distance tilted were averaged for the two speeds. On a given hour the trials were all lateral or all longitudinal.

Table II indicates the results briefly. Each row represents one hour's experiments. The first column gives the subject; the second the direction of movement; the third the percent superiority of the slow movement with respect to distance,

TABLE II

EFFECT OF DIFFERENT RATES OF MOVEMENT

		Distance		Time	
Subject	Direction	% superiority slow	Difference / P. E.	% superiority fast	Difference / P. E.
An	Lateral	72	5.9	132	6.8
An	Longitudinal	49	5.8	169	9.5
Ap	Lateral	54	4.9	160	7.3
B	Lateral	3	0.6	270	20.0
B	Longitudinal	23	2.8	224	9.5
C	Lateral	15	1.3	248	8.5
C	Longitudinal	195	10.7	36	4.3
O	Lateral	19	4.3	235	18.6
O	Longitudinal	—26	3.2	406	14.0
Sp	Lateral	212	7.7	27	3.1
Sp	Longitudinal	34	2.1	198	7.1
Average............		59.0	4.5	191.3	9.9

i.e., the percent by which the average distance in the slow trials is less than that in the rapid; the fourth this difference divided by its probable error; the fifth the percent of superiority of the trials at the more rapid rate with respect to the time; and the last the ratio of this difference to its probable error.

It is apparent that one reacts more quickly to the more rapid movement but actually tips farther from the horizontal. Two of the subjects, *An* and *B* give introspection corresponding to this fact. However with the speeds used the time factor

is much more pronounced than the distance,—191 percent on the average as contrasted with 59 percent. A detailed table shows further that the mean variation is appreciably greater on the slow than on the quick movement,—the average of the percents mean variation being 33 for the slow movement and 25 for the rapid. More errors are made on the slow movement than on the rapid,—11 percent of the total number of trials as compared with 7 percent. Thus it would appear that under the above conditions reactions to the more rapid movement are more reliable.

A further study of the main problem was made with the other equipment. The procedure was identical with the above except that the heavy spiral springs released by the ratchet were used instead of the relatively slow turning of the crank by hand. Whereas in the previous method the subject's "*Aufgabe*" was more analagous to threshold judgment, here it was more like choice reaction. Only one rate was used,—a tension on the spring which lowered the corner of the platform 3 cm. in 150 sigma. The motion was negatively accelerated. An electric vernier chronoscope or a 50th second stop watch operated by a magnet recorded the time. When the pawl left the ratchet a circuit was broken thus starting the chronoscope and the subject stopped it. The subject closed his eyes and pressed both keys down at the signal "ready."

TABLE III

DETECTION OF LATERAL VS. LONGITUDINAL MOVEMENTS AT RAPID RATES

Subject	Order	% superiority lateral	Difference / P. E. difference
An	Longitudinal	7	2.0
An	Lateral	22	5.2
Ap	Longitudinal	18	2.5
B	Longitudinal	13	3.2
C	Lateral	3	0.6
H	Longitudinal	4	1.2
H	Lateral	7	2.5
L	Longitudinal	21	6.1
L	Lateral	21	6.3
O	Longitudinal	—10	3.2
R	Longitudinal	9	2.2
R	Lateral	10	2.3
Sw	Longitudinal	3	0.8
Sw	Lateral	15	3.6
Sp	Longitudinal	10	1.7
Average		10.2	2.9

The lever was thrown about one second later and the subject lifted the proper finger according to the direction of the tilt. The noise of the apparatus of course informed the subject that he had moved but it was a case of choice reaction and there was no appreciable auditory difference between the two directions. The times show that the reaction sometimes followed after the platform had come to rest in a tilted position and sometimes before.

Table III is similar in form to Table I and gives the percent by which the average lateral reaction time is quicker than the average longitudinal and the ratio of the difference to the probable error of difference.

The same tendency is here again manifest. The differences are not as large as in the preceding method but are about 3 times the probable error and average 10 percent in favor of the lateral movement. The mean variations (not given in the table) are almost identical but slightly larger in the longitudinal. There are, however, considerably more errors on the longitudinal,— 5.8 percent of the total trials as compared with 2.4 percent. The results thus agree with the previous in showing a superiority of the lateral reactions.

The introspection of the subjects under these conditions is similar to that under the previous. *An* felt the movement largely in the head and considered the lateral a bit easier. *Ap* found the quick movement "more general and not localized in one part of the body." *B* felt it in the chair or the head. *H* felt pressure on the chair and "sensations as if the stomach were shrinking away from the clothes." The former was the more pronounced. He seldom noted sensations in the head. *L* felt the lateral "in the ears." The longitudinal was felt in the stomach and secondarily in the ears. *O* as previously judged almost entirely by tactual sensations. *R* felt it "almost entirely in the ears." *Sp* felt "nothing but the jerk." *Sw* reported the lateral as a bit easier. In general the same criteria seem involved in the more rapid movement as in the slower.

The conclusion seems justified that the average individual tilted from a horizontal position can detect a change in the lateral direction appreciably more readily (10 to 25 percent) than a change in the longitudinal direction.

In the above results the left and right reactions were averaged together and compared with the average of backward and forward. A further problem suggests itself as to left *vs.* right and backward *vs.* forward, and accordingly the results for each subject were studied with this question in

mind. As to the lateral reactions there is no appreciable difference. Taking the results for a given day and tabulating the percents by which the reactions to a left tilt are superior to those to a right, the results are as follows. For the slow movement, produced by turning the crank, the average superiority of the left, including negative values is 4 percent; for the choice reactions to the movement produced by the contraction of the spring the average difference is 5 percent; for the reactions with the dep control (cf. infra)—5 percent, and with the stick control 2 percent. In all cases there is a fairly equal distribution of plus and minus values. Tabulating similarly the percents of superiority of perception of forward as compared with backward, the average difference for the slow movements is 19 percent, with about three-fourths of the values tending in the positive direction. The results are especially marked in the case of one subject. Likewise with the dep reactions 6 out of 7 cases show a superiority of the forward with an average of 7 percent. The difference disappears however in the choice reaction with the keys to the rapid movement and in the stick reactions, the average percents being only 3 and 1 respectively. The probable errors are rather high, of course, because of the smaller number of trials represented by each percent, but there is a suggestion that, with some individuals at least, perception of forward tilt is keener than that of backward.

The above results are what might be expected if one assumes a genetic explanation for perceptions of this sort. In daily life one's static organs are swayed laterally from the perpendicular very frequently, e.g. in walking. They are less often moved longitudinally. One often leans forward at a table, etc., but less frequently does he lean backward from the perpendicular. If repeated movements of the ampullar and vestibular organs in a given direction increase delicacy of perception of such movements, we should expect to find the keenest perception for lateral movements with left and right about equally efficient and perception of forward somewhat more delicate than that of backward movements,—a result which is obtained empirically.[5]

[5]A fact of possible interest in the development of tests for aviators is the correlation between lateral and longitudinal reactions. If the lateral and longitudinal averages for all the different hours of experiment are ranked, they correlate (foot rule) to the extent of $.67 \pm .09$ for the slow movements and $.76 \pm .11$ for the rapid movements produced by the release of the spring. That is, an individual who has superior ability in one direction has it in the other, and one's diurnal efficiency in the various directions varies concomitantly.

IV. *Dep vs. Stick Airplane Control*

There are at present two types of airplane control in quite general use, and it seemed worth while to determine if there was any difference in a person's reactions with the two types. In the ordinary dep control a wheel is mounted vertically on a post between the pilot's knees and is grasped with both hands. Turning the wheel clockwise as viewed by the pilot banks the left wing, i.e., tilts the machine toward the right and vice versa. Moving the wheel forward, i.e., moving the upright on which the wheel is mounted, lowers the nose of the machine and vice versa. In the other control a straight stick is mounted in about the same position as the dep and grasped with one hand. Moving the top of the stick to the right lowers the right side of the airplane, moving it forward lowers the nose etc. In both cases the direction of the machine is controlled by a foot bar.

These conditions were duplicated as nearly as possible on the tilting platform. Standard specifications were observed with regard to the distance from chair to foot bar and controls, height of bar relatively to seat etc., so that the subject was in a bodily position quite similar to that of an aviator during flight.

The lateral and longitudinal experiments were made on separate hours so that a remounting of the apparatus was possible. In each instance a lever was attached from the control to the top arm of a cross-shaped upright. This upright was pivoted at the center of the cross and two fairly heavy spiral springs were fastened from the ends of the horizontal arms to the base. The lower arm closed an electric contact under slight friction when the arm was on a dead center. The springs held the cross on center and the controls in a normal position (i.e., vertical) unless the latter were moved by the subject. For the lateral movement the dep and stick were connected by a system of levers so that they moved synchronously. For the backward and forward movement they were rigidly connected, the only difference in reaction being the use of two hands on the wheel *vs.* one hand on the stick.

The experiments were conducted in much the same manner as the preceding. The movement of the platform was the same throughout,—the corner dropped with negative acceleration 3 cm. in 150 sigma. Trials with dep and stick were intermixed in a chance order during an hour,—about 30 of each. The subject sat with his hands in his lap until the experimenter said "dep" or "stick." He then grasped the

control indicated and closed his eyes. A final signal "ready" was given one second before the release of the platform from the horizontal. Between trials he moved the control back to the normal position. In the lateral series using the dep he turned the wheel toward the high side, i.e., if he tipped to the left turned the wheel clockwise or when using the stick moved the top toward the high side. On the longitudinal series using either control the motion was toward the high

TABLE IV

DEP VS. STICK

Force (pounds)	Subject	% superiority stick	Difference / P. E. difference
	H	—7.4	2.4
Dep........7.10	L	0.3	0.1
Stick.......6.65	R	4.5	1.4
	H	3.9	1.3
Dep........7.15	R	7.5	2.6
Stick.......6.65	Sw	—10.0	3.4
	Average	—0.2	1.8
	H	— 7.0	0.9
	H	—12.0	1.3
Dep........12.0	L	3.4	0.9
Stick....... 4.0	L	15.0	2.0
	R	21.5	3.8
	Sw	30.0	5.6
	Average	8.4	2.4
	H	14.0	2.8
Dep........20.0	L	10.2	2.1
Stick....... 5.5	R	49.0	7.4
	Average	24.4	4.1
Dep.........3.35	R	—10.8	3.7
Stick........9.35	Sw	—20.6	5.1
	Average	—15.7	4.4
	B	— 3.7	1.9
Longitudinal....	B	— 2.7	1.0
Both equal.....	R	10.3	4.8
	R	6.5	2.0
	Sw	8.0	2.8
	Sw	— 0.6	0.2
	Sw	— 3.5	1.1
	Average	2.0	1.9

end, i.e., when tipping forward the control was pulled backward. These are the movements made in actual flight. On a given trial there was simply a choice between two reactions. A very slight motion of the control broke the electric contact so that the beginning of the reaction and not its extent was recorded. The reaction time from the beginning of the tilt until the movement of the control was taken by the vernier chronoscope or the 50th second stop watch. On the lateral motion there was one other variable involved, viz., the amount of force necessary to break the contact. This was varied by changing the arrangement of levers so that the stick required a force greater or less than or approximately equal to that of the dep.

The results are summarized in Table IV. The first column indicates the number of pounds force required to break each contact, measured by applying a spring scale at the point usually grasped by the subject. The second column gives the subject; the third the percent by which the average reaction time with the stick on a given hour is superior to (i.e., quicker than) that with the dep; and the last the ratio of this difference to its probable error. Averages are given for each combination of forces necessary to move the controls.

The table shows that when approximately the same force is necessary to break the contact there is no general tendency and the average difference is 0.2 percent. When the stick moves more easily the reactions are quicker,—when the forces are 12 and 4 lbs. averaging 8 percent quicker and when 20 and 5 lbs. 24 percent quicker. When the dep moves more easily the opposite result is found. In the longitudinal experiments indicated at the bottom of the table where the two forces are equal there is no significant difference as dep and stick are each superior in about half of the trials. A survey of the mean variation and the percentage of errors (not given in the table) shows nothing of interest except that in one group (dep 20 *vs.* stick 5.5) there is a considerably higher variation in general on the dep.

The following points of interest were mentioned in the introspection. *H* in series with the stick moving more easily than or the same as the dep thought the reactions with the former were quicker but more apt to be wrong. With the stick he would "hold still and the body would do the rest." However "the stick seemed to have more control than the dep." During the fore-period he had a kinaesthetic image of both movements. *L* liked the stick better and held it upright and let the body do the rest. After practice however

he preferred the dep. Moving the stick to the right was easiest but moving it to the left was more difficult than the dep. *K*'s introspection followed directly with the objective inertia of the controls. When they were equal he thought there was little difference in his reactions; when the dep had greater inertia he reported quicker reactions with the stick and vice versa. However the "dep seemed steadier, i.e., using my two hands." *Sw* had a feeling of greater certainty with the dep. "When I reach out and take hold of it I have greater confidence. Think involuntary movements play greater rôle with stick. Body naturally goes in reverse direction and takes dep with it but with one hand it might jerk away." The most interesting point in the introspective records was the feeling of greater confidence with the dep control.

The conclusion seems justified that there is nothing in the nature of the reaction with dep as contrasted with the stick control that gives one any especial advantage over the other, provided the same amount of force is necessary in each case to overcome the inertia of the mechanism. Where such a difference in force exists the control requiring the least force yields the quickest reaction time.

Prior to the experiments under the above instructions the subjects were given a few trials with each control with instructions such as the following: "Grasp the wheel with both hands; one second after the warning 'ready' you will tip either to the right or the left; make as quickly as possible the reaction that seems most natural." With two of the four subjects who were tried thus on lateral movement there was a very marked tendency with both controls to react toward the high side as in an airplane. With the third this tendency was present with the stick but not with the dep. He spoke of associations with driving an automobile. The fourth subject reacted toward the low side with both controls. He had ridden a bicycle considerably and was reminded of it immediately and on a bicycle one steers toward the low side to maintain equilibrium. Only two subjects were tried with similar instructions on the longitudinal movement. They reacted immediately with both controls toward the high end. Thus it would seem that the types of reaction used in the standard airplane control are those which are more or less instinctive with the average individual and hence most readily automatized.

V. *Conclusions*

If an individual sitting in a chair on a platform which rocks about a point below its center is tilted from the horizontal

slowly at a constant rate until the direction is perceived, or performs a choice reaction to a rapid tilt with negative acceleration, (the type of movement experienced in an airplane). the perception of the lateral is considerably superior to that of the longitudinal movements. A forward tilt is slightly more readily perceived than a backward. This indicates that an aviator is slower in perceiving and correcting a pitch of his machine as contrasted with a roll. Hence in the selection or arrangement of airplane controls if those for one direction are to be more readily manipulated such should be used for longitudinal control to compensate the aviator's natural slowness in detecting a longitudinal motion.

When tilted at various rates (without acceleration) the direction is perceived more quickly at the faster speeds but one actually tips through a greater angle. However the time factor varies much more with change of speed than does the distance factor.

Judging by the introspection both tactual and static senses are involved in perceptions of this sort. Subjective estimates of efficiency under the various conditions do not correlate uniformly with objective results.

In reacting to sudden changes of equilibrium with standard dep and stick airplane controls there is no appreciable difference in the reaction times when the forces necessary to overcome the inertia of the mechanism are equal. Where such differences in force exist the reaction time is slower with the control possessing the greater inertia.

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AUDITORY ILLUSIONS OF MOVEMENT—A PRELIMINARY STUDY

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INTRODUCTION

The following study was suggested by the experiments of Wertheimer[1] and Korte[2] on visual illusions of movement and of Benussi[3] on similar tactual illusions.

Wertheimer exposed in quick succession two parallel lines a few centimeters apart. Under certain conditions of time and distance it was possible to produce the impression of a line moving in the same direction as the actual temporal succession of the stimuli (the ordinary illusion of the stroboscope and moving-picture). Wertheimer attributed the phenomena to a 'physiologische Kurzschluss' between two regions of the cortex corresponding to the two points in visual space. Korte repeated Wertheimer's work and, carrying the problem still farther, subscribed to the same theory. Benussi, working with two successive pressure stimuli, found somewhat similar results and also favored Wertheimer's theory.

It would seem interesting, inasmuch as the visual and tactual are fields in which there is some plausibility for a correspondence between points of objective space and points in the cortex, to apply the same methods in another sense-department such as audition, where such a correspondence seems highly improbable. Accordingly the present study

[1] Wertheimer, M., 'Experimentelle Studien über das Sehen von Bewegung,' *Zeitschrift für Psychologie*, 1912, 61, pp. 161–265.

[2] Korte, A., 'Kinematoskopische Untersuchungen,' *Zeitschrift für Psychologie*, 1915, 72, pp. 193–296.

[3] Benussi, V., 'Kinematohaptische Erscheinungen,' *Archiv für die gesamte Psychologie*, 1913, 29, pp. 385–388. Benussi, V., 'Kinematohaptische Scheinbewegungen und Auffassungsumformung,' *Bericht VI Kongres für experimentelle Psychologie*, Göttingen, 1914, pp. 31–35.

attempted to repeat the work of Wertheimer and Korte in certain of its aspects in audition.

These investigators found, for instance, that an interval of about 60 sigma between stimuli gave, on the whole, the optimal impression of movement. With longer exposure of the lines, however, the optimal interval was relatively somewhat shorter. Moreover, by increasing the intensity of the second stimulus or by voluntarily directing the attention to it, apparent movement was frequently produced in the reverse direction. Further studies were made with different combinations of lines and with variations of distance and intensity, but only three problems were taken up in the present case:

1. The possibility of auditory illusions of movement.
2. The relation between exposure and interval.
3. The effect of difference in the intensity of the two stimuli.

Technical difficulties made it inadvisable to proceed farther with the present form of apparatus.

Apparatus and Method

The stimuli consisted of telephone receivers in series with a 250 vd. tuning fork on a 5-volt circuit. Normally the current passed through the fork directly. When, however, one side of the line was broken by relays actuated by a time-controlling mechanism the current was shunted through one of the receivers which gave a faint buzzing sound of the same pitch as the fork. One receiver was mounted on an arm moving synchronously with the time-mechanism. Two others were stationary. It was thus possible to produce an actually moving sound, a moving one with a break in the middle, or two separate successive sounds in different positions. For the first two cases it was merely necessary to close switches so that a single relay was operated once or twice, the current in each case passing through the moving receiver. For the two stationary receivers connections were made so that when one relay broke the current passed from the fork through the one receiver and when a second relay broke, the current

passed through the other receiver. Rheostats in series with all the receivers regulated the intensity of the stimuli.

A wooden disc, mounted upright, was rotated by an alternating-current motor reduced by belt gear and speed reducers. Various speeds were obtained by different combinations of gearing. Four brass contacts were clamped over the edge of this disc and held in place by set-screws on the rear side. Nearer the center were four concentric brass rings a few millimeters apart. These were connected through the disc by means of flexible wire to the rear of the four brass contacts. Brushes of spring brass pressed upon these rings, thus affording connections with the four contacts while the disc was rotating. A common pole of spring brass on one side of the main line brushed across the four contacts in succession. These latter passed to various connections operating the magnets of the relays. A telegraph key in series with the main brush enabled the whole line to be thrown on or off at any instant.

In some cases only two contacts were used, the width of the contacts giving the length of time the relay was broken, and hence the length of the sound, and the distance between the contacts giving the interval between the two sounds. For a wider range of adjustments a relay had the spring removed and a second set of magnets on the opposite side of the armature adjusted to just touch the armature when it was in its normal position. The residual magnetism of the cores held the armature there even though the relay was tipped slightly so that the armature tended to fall. The first contact on the disc (when the main brush passed across it) actuated the original magnets, pulling the armature down. It stayed there until the second contact actuated the other magnets pulling the armature back and holding it there by residual magnetism. The third and fourth contacts produced the same effect on a second relay arranged in the same manner. Thus by varying the distance between contacts and the speed of the disc, any desired exposure or interval could be obtained. The exposures and intervals were roughly calibrated by sending the alternating current with lamp resistance through

the relays and a signal magnet writing on a kymograph. This could be read accurately to 1/120th of a second. In some of the work the Hipp chronoscope was also used to measure the time.

The experiments were performed in the sound-proof room of the laboratory. The subject and the stimuli were in an inner room behind double doors. The stationary receivers were hung on the edge of a sound cage one meter in diameter. The subject sat either in the center of the cage or at a distance of two meters from the receivers. In some of the work a special rack with a black cambric screen between the receivers and the subject was used. The moving receiver was fastened to a wooden arm, the lower end of which was pivoted to a rod attached to the table edge. A horizontal rod pivoted to this arm passed through a small hole in the wall and was connected to a shaft operated by a crank attached to the rotating disc of the time-controlling mechanism. Thus the receiver oscillated synchronously with the exposures of the sounds through a distance of 70 cm. The receivers were all on the level of the ears of the subject who sat facing them on an adjustable chair. Preliminary experiments with the subject facing in various directions appeared to yield no significant facts except the more frequent confusion of direction of motion when the sounds were directly at one side, *i. e.*, at the end of the first or third quadrants, considering the eyes as facing zero. This would correspond to the usual confusion in the direction of sounds localized symmetrically with reference to the plane passing vertically through the two ears. The motor and tuning fork were in padded boxes so that the sound was minimized and what passed through into the inner room afforded little distraction but served rather as a warning signal.

At the beginning of the hour's work the receivers were equated in intensity according to the judgment of the subject. He was then simply told that when the light was extinguished he was to give his attention in the direction of the receivers and after the doors were again opened to describe what he had heard. The doors were then closed,

the fork started, the light extinguished and the time mechanism set in operation. Some time between the passing of the fourth and first contacts across the common brush the telegraph key was depressed and held down until the fourth contact had again passed, thus giving the stimuli in the desired arrangement. This arrangement was varied from day to day, sometimes continuously moving sounds intermixed with broken movement, sometimes moving sounds mixed with stationary ones.

Five subjects participated in the experiment, all graduate students with psychological training (one a woman). Only one was at all familiar with the problem and none were informed of the trend of results through the work, although all realized the difficulty of judging and were curious as to their efficiency. The experiments were performed in the Harvard Psychological Laboratory in the first half of the college year 1915–16.[1]

The present study is only of a preliminary nature. The stimuli were far from satisfactory. Although the intensity of the receivers could be adjusted by the rheostats there were invariably differences in timbre. These could be regulated to some extent by screwing the cap which confined the edges of the diaphragm. But undoubtedly the subjects almost always had timbre differences to assist them in distinguishing the discreteness of the two sounds, and hence the illusion took place less frequently and consistently. It would seem desirable to do more exhaustive work with tandem-driven tuning forks in boxes with small apertures, controlling the intensity by rotating the forks.

Results

The principal results for experiments with two sounds of equal intensity are summarized in Table I. The main interest lies, of course, in the trials in which two successive stationary sounds in different places yielded an impression of movement. The actually moving sounds and the broken moving sounds were introduced mainly as a check and to

[1] The writer expresses his obligations to the late Hugo Münsterberg.

avoid the subject's anticipating the character of the stimulus. These are not included in the table. The first few columns give the conditions investigated which yielded the optimal impression of movement for each subject; the next group those conditions in which the two sounds were always recognized as discrete and in different positions; and the last group those conditions in which this latter was nearly always the case but the illusion occasionally present. The first column in each group gives the time relations, *i. e.*, the time of the first sound, of the interval and of the second sound, in sigma. The second gives the distance apart of the receivers and the next the distance from the receivers to the subject's head, in centimeters. The fourth column in the first group indicates the per cent. of trials yielding the impression of movement, this percentage being based on from 10 to 20 trials. About this same number of trials forms the basis for the conditions in the other columns. There were few cases in which a single sound was heard. In the visual experiments the two lines were often seen simultaneously. In the auditory field this simultaneity would naturally resolve into a single sound midway between the sources. This occasionally happened, usually with intervals of 10 sigma or less, and the timbre differences in the receivers militated against such results.

The table shows that, under certain conditions of time and distance, two similar sounds in different positions with a definite time interval between, give the impression of a single moving sound.

It is to be noted first of all that one of the subjects does not appear in the table. This subject never reported movement on any of the trials with two separate sounds. Her auditory acuity was very good, according to her own statement, and it was quite manifest in the present case. Even with a single receiver moving and the sound broken in the middle of the movement she was seldom deceived, apparently catching the click as the diaphragm made its first contact with the magnet. On the last hour's experiment, however, as a result of suggestion she reported the illusion frequently. The experimenter intentionally expressed surprise at the

TABLE I

Subject	Optimal Movement: Time	Distance			No Movement: Time	Distance		Seldom Movement: Time	Distance	
A.....	190-33-190	18	200	80%	150-100-150	27	50	190-33-190	27	200
	76-10-76	18	200	75%	225-25-225	18	200	175-30-175	9	200
	76-10-76	27	200	100%	120-70-120	27	50			
	76-8-76	18	200	50%	120-48-120	20	200			
	68-54-68	18	200	85%	114-15-114	9	200			
					105-85-105	18	200			
					80-70-90	27	50			
					72-28-72	20	200			
					60-60-60	20	200			
Bu....	90-10-90	18	200	66%	175-30-175	18	200	150-90-150	21	50
	67-6-67	18	200	50%	135-12-135	18	200	114-15-114	27	200
	47-38-47	18	200	66%	105-18-105	18	200	47-68-47	27	200
	47-38-47	27	200	50%	100-60-100	18	50			
	47-32-47	18	200	75%	79-55-79	18	50			
	47-32-47	27	200	75%						
	37-30-37	18	200	50%						
	37-30-37	27	200	66%						
C.....	225-25-225	27	200	66%	120-100-120	18	50			
	190-25-190	27	200	100%	90-10-90	27	200			
	180-48-180	10	200	100%	80-70-80	18	50			
	150-30-150	9	200	100%	70-80-70	18	50			
	150-30-150	18	200	100%	68-54-68	27	200			
	150-30-150	27	200	75%	66-100-66	18	200			
	150-30-150	36	200	66%	50-33-50	18	200			
	144-54-144	50	200	100%	33-25-33	18	200			
	144-54-144	60	200	100%						
	144-28-144	50	200	60%						
	114-15-114	27	200	100%						
	114-15-114	18	200	100%						
	108-28-108	10	200	75%						
	76-10-76	27	200	100%						
	72-28-72	10	200	66%						
	72-28-72	20	200	62%						
	67-7-67	27	200	85%						
	47-38-47	18	200	86%						
P.....	190-25-190	18	200	75%	175-30-175	18	200	150-90-150	21	50
	135-12-135	18	200	66%	120-48-120	10	200	144-54-144	10	200
	114-15-114	36	200	100%	100-70-100	18	50	144-54-144	20	200
	76-10-76	18	200	71%	79-55-79	18	50	144-54-144	30	200
	76-10-76	27	200	50%	76-16-76	18	200	144-54-144	40	200
	68-12-68	18	200	50%	58-68-58	9	200	144-36-144	10	200
					58-68-58	18	200	144-36-144	20	200
					58-68-58	27	200	144-36-144	40	200
								105-18-105	18	200
								90-8-90	18	200
								90-8-90	27	200
								72-28-72	10	200
								60-48-60	10	200
								67-6-67	18	200
								38-28-38	10	200

fact she never noted the actual motion of the sound, whereupon she did note it repeatedly.[1]

There are further individual differences evident. *C* manifests the illusion to a far greater extent than any of the others, reporting it not only under more settings but also in a larger per cent. of trials on a given setting. In some instances the illusion was successful with the receivers 50 or 60 cm. apart. He stated that it was 'usually easy to tell which way they go and to tell one from two.'

P's results, on the other hand, fall mostly in the group in which the illusions occurred but seldom on a given setting. He often noted however a peculiar 'feeling of movement' in addition to the perception of the two discrete sounds. To quote his introspection: "Sometimes get two sounds but a distinct feeling of movement. I rationalize it with the stick moving and so get a feeling of movement." Or again: "Two sounds but an impression of movement besides. Get idea it is the same sound travelling. Have often some visual scheme, *e. g.*, like sticks on a fence. The feeling itself unanalyzable, like the feel of a band going down the street; sort of an interpretation of the thing." He noted at another time that, "If the sounds move at about the proper rate to coincide with what I think is moving I get the impression of movement."

A reported the illusion in only a few conditions and these were mainly near the outset of the experiment. Toward the end he became sceptical. He seemed to judge much by the after-image. The following introspections are typical: "Judgments afterward, like seeing a mass of things on a screen and analyzing it;" "Distinct image afterward and look at the sound picture." Toward the end of the experiments he admitted that he was sceptical and thought 'the apparatus fitted for only two sounds.'

Bu showed the illusion in a number of settings. He noted

[1] This effect of suggestion in producing the illusion indicates the advisability (with some subjects at least) of proceeding as in the present case without explaining the nature of the experiment, rather than following the methods used in the visual studies where the subject knew all about it and was required to state whether he perceived the illusion or not.

that it was easier to tell the direction in which the sound was moving than to tell one from two sounds. He found that much too depended on the attention. "If you wait too long it catches you on the wrong wave."

It is to be noted further that the illusion almost never occurred with the subject in the sound-cage, *i. e.*, 50 cm. from the receivers. Presumably the intensity of the stimuli was too great or their timbre differences too pronounced at that distance, even though the current was reduced as far as it was possible to do and still keep the fork going.

Wertheimer and Korte found that in general an interval of some 60 sigma between the two stimuli a few centimeters apart gave the optimal impression of visual movement. The above table would indicate a somewhat shorter interval as yielding optimal results in the auditory field,—ranging from approximately 10 to 50 sigma with the average 25 or 30. This may correspond to the usual finding that auditory reactions are quicker than visual.

The above investigators also found that the longer the exposure the relatively shorter is the optimal interval. A glance at the first column of the table shows that such was the case in the present study. The rows in the column are arranged for each subject in order of magnitude of the *exposure* with the longest at the top. It will be seen by following the *intervals* down the column, that in many cases they grow actually longer as the exposure decreases and in almost all cases relatively longer. There were a number of instances in which trials were made on a given subject with the exposure and interval in the same ratio while the absolute values were changed. Such series are grouped separately in Table II. The designations of the separate columns are the same as those of the first two divisions of Table I., but figures grouped together in a given row are those in which the ratio of interval to exposure time is constant. For example the first row indicates that with exposures of 76 sigma and an interval of 10, apparent movement was often perceived, while with exposures of 114 and interval of 15, which was in the same ratio as the first conditions, movement was never apparent. It is evident

that with three of the four subjects, given exposure and interval in the same ratio, the longer absolute values yield no movement while the shorter do. That is, the longer the exposure, the relatively shorter is the optimal interval.

TABLE II

Subject	Optimal Movement			No Movement		
	Time	Distance		Time	Distance	
A....	76-10-76	18	200	114-15-114	9	200
	76-10-76	27	200			
B....	67-7-67	18	200	135-12-135	18	200
	47-32-47	18	200	79-55-79	18	50
	47-32-47	27	200			
C....	225-25-225	27	200	90-10-90	27	200
	47-38-47	18	200	68-54-68	27	200
				33-25-33	18	200
P....	68-12-68	18	200	175-30-175	18	200

The other factor of interest in the present study was the effect of varied intensity. It was found in the visual experiments that, given the optimal conditions for apparent movement, increasing the intensity of the second stationary stimulus produced apparent movement in the reverse direction. This factor was studied in the present case for a few of the optimal time relations by adjusting the rheostats in series with the receivers. The sounds were equated in intensity at the beginning of the hour's work according to the judgment of the subject, and during the hour the rheostat in series with the second receiver was varied. The intensity of the sound was not measured, but four different settings of the rheostat were intermixed irregularly with the normal setting. Table III. gives the results found in this manner with two stationary receivers and the intensity of the second sound equal to or greater than that of the first. The first columns give the time of exposures and intervals and the distance of the receivers from one another and from the subject as in Table I. Then follow the per cent. of trials in which normal and reverse movement was produced in trials with the intensities equal and in trials with the second sound louder than the first.

Normal movement, of course, signifies apparent movement in the direction in which the temporal succession of the sounds actually occurred. The results in a given row in the table were always obtained on a single day.

TABLE III

				Equal Intensity		Second Sound Louder	
	Time	Distance		Normal Movement	Reverse Movement	Normal Movement	Reverse Movement
A............	190-33-190	18	200	60%	20%	0%	50%
	190-33-190	27	200	40%	0%	40%	20%
	76-10-76	18	200	75%	0%	27%	18%
	76-10-76	27	200	100%	0%	0%	0%
	76-8-76	18	200	50%	0%	60%	20%
B............	67-6-67	18	200	25%	25%	0%	66%
	47-38-47	18	200	50%	0%	0%	0%
	47-38-47	27	200	0%	66%	0%	100%
	47-32-47	18	200	50%	25%	0%	25%
C............	190-25-190	27	200	50%	50%	33%	66%
	114-15-114	27	200	66%	33%	43%	57%
	108-28-108	10	200	75%	12%	0%	60%
P............	135-12-135	18	200	66%	0%	66%	0%
	90-8-90	18	200	40%	0%	0%	0%
	67-6-67	18	200	33%	0%	0%	0%
Average.......	....	...	......	52.0%	15.4%	17.9%	32.1%

It is quite evident from the averages that the reversal effect is often produced by an increase in the intensity of the second stimulus. Whereas with stimuli of equal intensity the illusion of movement in the same direction as the actual temporal succession occurs in 52 per cent. of the trials and in the reverse direction in only 15 per cent., with the second stimulus louder the illusion in the normal direction occurs in only 17 per cent. of the trials and in the reverse direction in 32 per cent.

SUMMARY

1. The presentation of two faint similar auditory stimuli in quick succession a few centimeters apart yields, under certain conditions, an impression of a sound moving in the direction of the actual temporal succession of the stimuli. Individuals vary in their susceptibility to the illusion but four of the five subjects used manifested it at various times.

For the fifth subject imperfect technique combined with marked auditory acuity perhaps accounts for the results. A time interval between the sounds of 25 to 30 sigma yields on the whole the optimal impression of movement.

2. There appears a rather definite relation between the length of the period of exposure and of the interval between the stimuli. The longer the exposure, the relatively shorter must be the time interval to yield the optimal impression of movement.

3. If the intensity of the second stimulus is greater than that of the first the apparent movement is often in the reverse direction.

Conclusions

As far as the experiment was carried, the results were strikingly similar to those of Wertheimer and Korte. This fact necessitates a reconsideration of Wertheimer's theory. The fundamental assumption of this theory is that stimuli at various points in visual space are correlated with disturbances in corresponding regions of the visual cortex and that there is a 'physiologische Kurzschluss' between these cortical regions. Corresponding to this Kurzschluss there is the perception of (illusory) movement between the original external points. Increasing the intensity of the second stimulus can sometimes reverse the direction of this 'Kurzschluss.' The finding by Benussi of similar results in tactual space does no violation to the theory, for a similar assumption may be made of a correlation of different points on the skin with different points in the cortex and of a physiological 'Kurzschluss' between such cortical regions. But with audition it is a different matter. The auditory end organs are not stimulable at different points as are the retina and the epidermis. There is no evidence for the correlation of separate points in auditory space with separate regions of the sensory cortex. Wertheimer's theory cannot explain the results of the auditory experiments as it does those of the visual. Yet the same subjective phenomena are manifested in both cases. The implication is that there must be some additional factor operative in both audition and vision.

The writer is inclined to explain the phenomena on the basis of the 'action theory.' A sound at the level of the ears produces, by its binaural intensity difference, a motor impulse to turn the head or eyes in its direction. If a second sound in the same plane and at the same height supervenes shortly, there is a second motor impulse, presumably in the same centers, before the first is completely exhausted. The position of the sounds may thus be represented cortically by impulses of different intensity in the motor regions leading to the same muscles of the eyes or neck. If the second sound supervenes rapidly enough there is a continuity of the motor impulse. The direction in which the second impulse would lead, if executed, relative to the first, gives the cue as to the direction of motion. With the increase of intensity of the second stimulus it is conceivable that the second motor impulse is temporally facilitated sufficiently to produce the effect which would have obtained had it actually preceded. It is well known that reaction time decreases with the increase of the intensity of the stimulus.[1]

Some such factor as the above may well be involved in the similar visual and tactual phenomena. It seems probable that a visual or tactual stimulus produces an impulse to make some muscular adjustment in order to more clearly perceive the source of the stimulus. The biological importance of such a tendency is obvious. A second stimulus may produce an additional impulse before the first is exhausted and the continuity of the two gives the illusion of movement. At least it seems that a different theory from Wertheimer's is necessary to account for the auditory illusions. Perhaps this same theory may be involved in the visual and tactual inasmuch as they manifest the same characteristics. The writer hazards the belief that it is a question of the continuity of the motor impulses.

[1] Cf. Pieron, H., 'Recherches sur les lois de variation de temps de latence sensorielle en fonction des intensites excitatrices,' *L'Annee Psychologique*, 20, 1914, 17-96.

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TACTUAL ILLUSIONS OF MOVEMENT[1]

BY HAROLD E. BURTT

A. INTRODUCTION

The kinematoscopic illusion in which two similar stationary visual stimuli in quick succession in different places yield an impression of movement has long been familiar. Wertheimer[2] and Korte[3] studied the phenomenon rather exhaustively with variations of intensity, distance, interval, etc. The writer has demonstrated the possibility of such illusions in audition.[4] Benussi has found similar phenomena in touch and has published two brief accounts,[5, 6] the latter of which appeared after the present study was in progress. It seemed profitable to determine whether the tactual corresponded to the other sensory fields merely in the existence of the illusion under optimal conditions, or whether there was a correspondence in the more detailed aspects. Accordingly an investigation was made of the rôle of the intensity of the tactual stimuli, the distance between them and other variables much as they were studied by Korte in the visual phenomenon.

The principal facts found by Korte are as follows:

1. If two equal straight lines or other similar visual stimuli a few centimeters apart are presented in quick succession for equal lengths of time certain conditions of intensity, time and distance yield an impression of movement from one

[1] From the Harvard Psychological Laboratory.

[2] Wertheimer, M., 'Experimentelle Studien über das Sehen von Bewegung,' *Zeitschrift für Psychologie*, 1912, 61, pp. 161–265.

[3] Korte, A., 'Kinematoskopische Untersuchungen,' *Zeitschrift für Psychologie*, 1915, 72, pp. 193–296.

[4] Burtt, H. E., 'Auditory Illusions of Movement,' J. OF EXP. PSYCHOL., 1917, 2, pp. 63–75.

[5] Benussi, V., 'Kinematohaptische Erscheinungen,' *Archiv für die gesamte Psychologie*, 1913, 29, pp. 385–388.

[6] Benussi, V., 'Kinematohaptische Scheinbewegung und Auffassungsumformung,' *Bericht VI. Kongres für experimentelle Psychologie*, Göttingen, 1914, pp. 31–35.

stimulus to the other in the direction of the actual temporal succession.

2. The longer the exposure of the stimuli the relatively shorter the interval which gives the optimal impression of movement.

3. The greater the distance between the stimuli the greater the optimal interval.

4. The greater the absolute intensity of the stimuli the shorter the optimal interval.

5. The greater the intensity the greater the optimal distance.

6. If the second stimulus is of greater intensity than the first the direction of the apparent movement is sometimes the reverse of the actual direction of temporal succession.

The above relations were investigated in the present case with tactual stimuli. The effect of varying degrees of difference in intensity between the two stimuli, which occupies a considerable portion of Korte's monograph, was not studied inasmuch as the reversal effect occurred much less frequently in the present case.

B. Apparatus and Method

An experiment of this sort necessitated some method of giving two similar tactual stimuli at different points of the skin, with control of the time relations, intensity of the stimuli and distance between them. It was also desirable to give as a check some stimulus approximating in its effect a continuous movement on the skin.

Ten brass rods with blunted points 2 cm. apart in a straight line were arranged to stimulate the back of the forearm. With the whole row actuated by solenoids in quick succession the effect was always much like a continuous movement, for any two adjacent points were within a "Weber's circle." This gave the check mentioned above. For most of the work only two solenoids were actuated on a given trial, the distance between stimuli depending on which solenoids were used.

The solenoids were mounted individually on narrow strips

of wood arranged to slide vertically side by side on a large wooden base. A piece of round magnet iron 2.5 cm. long formed the core and was suspended by a spiral spring from an adjusting screw above the solenoid. The lower end of this core contained a hole into which a brass rod of the same diameter as the core was dowelled. The lower end of this brass rod was pointed to give the tactual stimuli. The subject's arm was clamped palm downward in the rest of the Mosso's ergograph with the first and third fingers in the usual tubes to insure rigidity. The base to which were attached the strips of wood carrying the solenoids was mounted longitudinally above the arm. These strips could be moved up and down individually by set screws. The base also could be moved vertically so it was possible to adjust the 10 brass points to any desired distance from the skin. Any hairs in the vicinity of the brass points were of course removed. The iron part of the cores were usually set a few millimeters above center and the brass points adjusted to a distance of about 1 mm. from the skin. Thus the intensity of the stimulus did not vary appreciably with difference in length of time the current passed through the coils although it did vary with the intensity of the current. This latter was controlled by rheostats. The solenoids were actuated by either about 20 volts of storage batteries or by the direct 110-volt laboratory current with a rheostat in series.

A time-controlling mechanism was made from the 'Leipzig time sense apparatus' with a series of closely adjacent marginal contacts and a rather complicated switchboard. It was possible to actuate the 10 solenoids in rapid succession or any two in succession for desired lengths of time with any desired interval between. The space order (up or down the arm) could be reversed at will. The time mechanism was placed in an inner soundproof room. The experimenter and subject sat in an outer room at opposite sides of the table on which were the solenoids, switchboard and rheostats. The subject's arm rested comfortably in the apparatus which was so placed that the upright base was between the subject and the solenoids. Their action was thus invisible

to the subject but could be watched by the experimenter. The action of the solenoids was practically noiseless.

The subjects were not told of the nature of the experiment but merely that it was a study of tactual perception. At the beginning of each hour they were instructed: "When I say ready close your eyes and attend to the forearm and then tell what you felt."

The warning 'ready' was given approximately 1 second before the stimuli. On a given hour trials involving different relations of the given variables were presented in irregular orders. The main interest was in the effect of two successive stimuli, but trials were always intermixed in which a row of adjacent stimuli were given. It could thus be seen how the subject would confuse two discrete stimuli with the close approximation to continuous movement produced by a row.

The experiments were performed in the Harvard Psychological Laboratory in the academic year 1915–16. The subjects were graduate students or undergraduates of considerable psychological experience.

C. Results

I. *The Movement Illusion*

All four subjects who participated in the experiment yielded in many trials the illusion of movement. If the point of a rod was pressed on the skin of the forearm for a definite period and then after a brief interval another rod a few cm. distant pressed on the skin for the same length of time as the first, there was sometimes an impression of movement from the first point to the second, sometimes a single fused sensation and sometimes two discrete impressions. These effects varied with the combination of time, intensity and distance variables employed (*cf.* infra).

The (illusory) movement impression was variously characterized in the introspection of the subjects.

Subject *A* distinguished a *series* in which there appeared to be a number of stimuli in a row 'pretty close but possibly felt as separate,' from a continuous *flow* from one end to the other in which he 'could not tell the separate points.'

There was also at times an *arrow* at the end of the flow as if the movement went on in the air a short distance.

C noted three forms of the illusion: a *line* which was 'as if you pressed down a long object' either all at once or beginning at one end of the object; a *walk* in which there are a number of discrete points in succession; and a *roll* which is 'continuous movement.' He also occasionally reported a *loop*, the motion being in the air above the arm rather than actually upon the skin.

D described the apparent movement as a *snake effect*. He felt a number of intermediate points but also a connection. It suggested black points connected by a line; 'when they were close together the line was thick and when far apart it was thin.' The effect was best when all points appeared equidistant. A frequent report was three points in succession in the order *ABC* or *ACB*. In the latter case, with the middle point felt last, the snake effect appeared less readily.

F reported movement in which they 'skip along like playing four or five notes on a piano in succession.' It often suggested a little boy running along. He also noted frequently a sort of *loop* in the air between the ends. This 'blends into a sensation of movement.' This loop reported by *C* and *F* corresponds to Benussi's 'Bogenbewegung in der Luft.'[1] Benussi found that with a greater time interval the Bogen was greater and the movement worse. This seemed true for the present subject *F*.

II. *Exposure and Interval*

Korte found in his visual studies that the longer the exposure of the stimuli the relatively shorter the interval that yielded the optimal impression of movement. The writer found the same to be true with auditory stimuli.

To test this factor in the present case, intervals between stimuli of 15, 21, and 40 sigma were used with exposures of the same length or multiples thereof. The distance between the stimuli was constant at 12 cm.; the intensity was moderate, that given by a current of 1.5 amps. in the solenoids.

[1] *Op. cit.*, p. 32.

The results are summarized in Table I. The three successive columns represent results obtained with intervals of 15, 21 and 40 sigma respectively. At the left of each column are arranged vertically the exposures (in sigma) which were

TABLE I

EXPOSURE AND INTERVAL

(Distance 12 cm. Intensity 1.5 amps.)

Subject	Exposure	Interval 15	Exposure	Interval 21	Exposure	Interval 40
A			126	Two ends more discrete		
			105	Two ends	200	Two ends rather close together
			84	Series or two	160	Short series then other end
	45	Series or flow	63	*Series or flow*	120	Series three or four
	30	Two adjacent	42	Slight flow	80	*Four in flow or series*
	15		21	Single one in middle	40	Three in series
C			147	Two or three at each end		
	90	Ends successive	126	Five rolling with gap	240	*Four to six in succession*
	75	Ends successive	105	*Six or seven in succession (walk)*	200	Three or four in succession
	60	Four or five in succession (line) or two ends	84	One end then three rolling at other end	160	Three in succession
	45	*Four in succession (line)*	63	Three in succession	120	Ends successive but spread
	30	Ends successive	42	Ends successive or simultaneous	80	Ends successive but spread
	15	Ends successive or simultaneous	21	Ends successive or simultaneous	40	Ends successive
D			126	Group at ends		
			105	Two or three in series	200	Several at each end
			84	*Three or four with heavy line*	160	Three or four in series
			63	Three with thin line	120	Three or four in series
			42	Several simultaneous	80	*Four with snake effect*
					40	Three or four in series or ends
F	60	Ends	84	Ends		
	45	Ends	63	*Three or four*		
	30	*Three or four*	42	Two or three		
	15	Two adjacent	21	Two close together		

combined with the given intervals. In each section is stated briefly the phenomenon that generally occurred with that given combination of exposure and interval for the subject

indicated in the margin at the left. Each statement is based on from 5 to 10 trials and all results in the table were obtained on the same day. The check trials in which an actual series of touches was given by several successive solenoids are omitted. The results in this and subsequent tables are for only those trials in which two stimuli were given for equal lengths of time with an interval between. For instance we note that with the stimuli for 30 sigma each with an interval of 15 sigma between them, subject *A* generally reported two adjacent touches, and with stimuli for 42 sigma each and an interval of 21 sigma he noted usually a 'slight flow.'[1] Following across any horizontal row in the three columns we have exposure and interval in the same ratio. Thus in the example above 30 : 15 : : 42 : 21 : : 80 : 40. That report in each column which seems to indicate the greatest effect of the illusion is indicated in italics. Thus by noting the distribution of the italicized phrases for the different subjects any individual or general tendencies may be seen. The variables are arranged (in all tables) with the lowest values at the lower left corner so that an array of italics extending obliquely upward to the right indicates a positive relation between the variables and an array extending obliquely downward to the right indicates an inverse relation.

In the table it is evident that two of the subjects show a direct relation and two an inverse relation between the exposure and interval which give the best impression of movement. For instance with subject *A* an interval of 21 sigma is best combined with exposures of 63 sigma, 3 times as great, while a longer interval (40 sigma) is best combined with one only twice as great (80 sigma). That is, the longer the interval the relatively shorter the optimal exposure. *D* shows similar results. *C* and *F* manifest the opposite tendency but the latter's difference is very slight (a matter of 3 *vs.* 4 apparent stimuli) and perhaps negligible. On the whole the difference appears slightly in favor of the inverse relation. The longer the exposure of the stimuli, the relatively shorter the optimal interval. One may note further

[1] For explanation of individual notations, see p. 374–5.

that for a given interval the shorter exposures give an impression of simultaneity or fusion into a single touch, while increase of the exposure produces the movement illusion and still further increase an impression of discrete successiveness, a result in accord with Korte's visual findings.

III. *Distance and Interval*

Table II. summarizes the results with the distance between the points on the skin and the time interval between the stimuli constituting the variables. The exposures were constant at 60 sigma and the intensity at 1.5 amps. through the solenoids. The successive columns give results with the lengths of interval indicated. The rows give the different distances employed. The general tendency for each combination of interval and distance is indicated by a phrase as in Table I., and the report in each row showing the clearest evidence of the illusion is italicized, where such evidence is present. The results for *F* are indicated numerically rather than qualitatively. He usually gave the numbers of the points which he thought touched the skin (having adopted a scheme with No. 1 next the wrist, etc.). From three to six points were reported in each trial. These were simply added and averaged and the figure in the table represents the average number of successive stimuli reported per trial on the given combination when only two stimuli were actually given.

A glance at the italics shows in every instance a tendency toward a distribution obliquely upward to the right. That is, to obtain the optimal impression of movement the longer the interval between the stimuli the greater must be the distance. This result is the same as that found by Korte in vision. We may note further that for a given distance small intervals give a single point or simultaneity, while increase of interval gives movement and later discreteness and succession.

IV. *Absolute Intensity and Interval*

The intensity of the stimuli was varied from 1.8 amps. through the solenoids, which gave a moderate touch to 4.4

TABLE II

DISTANCE AND INTERVAL

(Exposure 60 sigma. Intensity 1.5 amps.)

Subject	Distance	Interval: 15 Sigma	30 Sigma	45 Sigma	60 Sigma	75 Sigma	100 Sigma
A	16 cm.		Ends or flow at each end;	Ends	Ends	Ends	Ends
	12 cm.		1 spread out	2 adjacent; with arrow	2 or 3 adjacent;	*3 in series*;	
	8 cm.		1 or short flow	1 or short flow	*Flow*	Flow or series	Series of 3 or 4
A (later)	16 cm.	Ends	Ends	Ends	Ends	Ends	
	12 cm.	Short series or flow	Series or 2 adjacent;	*Flow or series*	Two	Two	
	8 cm.	*Flow*	Series	Series or 2 adjacent;	2 adjacent;	2 adjacent;	
C	16 cm.	Ends successive or simultaneous	Ends or series	Ends or series simultaneous	Ends each spread	Ends each spread	Ends
	12 cm.	Ends	Ends	2 or 3 successive or simultaneous	*4 in succession*	*4 in succession*	Ends spread
	8 cm.	Several in succession or simultaneous	3 in succession	*3 or 4 in succession*	*3 or 4 in succession*	3 in succession	3 in succession
D	16 cm.	Ends	Ends	Ends	*Slightly many*	Ends	
	12 cm.	Several simultaneous	Several simultaneous	*Snake*	*Snake*	2 or 3 in series	
	8 cm.	One	One	One	One	2 near together;	
F	16 cm.	3.2		*4.8*		4.0	
	12 cm.	*7.2*		5.8		4.8	
	8 cm.	*5.0*		4.2		4.4	

(Exposure 120 sigma. Intensity 1.5 amps.)

Subject	Distance	40 Sigma	80 Sigma	120 Sigma	160 Sigma
C	16 cm.	Discrete groups;	*Discrete groups walking or rolling*	Ends slightly spread	Ends slightly spread
	12 cm.	*5 or 6 rolling*	4 or 5 walking; or line	Discrete groups;	Discrete groups walking
	8 cm.	Several walk	Several walk or roll	Several walk with gap	Several walk

amps., a stimulus so intense as to be startling. In the separate columns of Table III. are the results with different intensities. The rows give the intervals. The exposure was held constant at 60 sigma and the distance at 12 cm. The

Table III

Intensity and Interval

(Distance 12 cm. Exposures 60 sigma)

Subject	Interval	1.8 Amps.	2.1 Amps.	2.4 Amps.	3.2 Amps.	4.4 Amps.
A	75 sigma	Two adjacent	Two adjacent	Ends	Ends	Ends
	45 sigma	*Two adjacent or flow or arrow*	*Two adjacent or flow or series*	Two or series	Two quite close	Two farther apart
	15 sigma		Series or flow	*Flow*	Two adjacent	Two farther apart
C	75 sigma		Several in row	*Several in row or line*	Ends or row with loop	Ends or row with loop
	45 sigma		One or two	*Several in row sometimes loop*	Several in row	Several in row or ends
	15 sigma		Several in row or one	Several in row	*Series with loop or lot of motion*	Series or ends
D	75 sigma		2.0	2.2	3.4	*3.8*
	45 sigma		1.6	2.4	3.0	*3.4*
	15 sigma		1.6	2.0	2.6	*3.8*
F	75 sigma		4.2	*4.8*	3.8	4.4
	45 sigma		4.4	4.6	*5.2*	*5.2*
	15 sigma		4.4	6.6	*5.6*	*7.0*

general tendency shown on each combination is briefly indicated as in previous tables and the phrase in each row which indicates the optimal illusion italicized. The results for *D* and *F* are recorded numerically as in Table II.—the average number of touches felt per trial.

The italics are distributed with three of the four subjects in a direction obliquely downward toward the right. With *D* the greatest movement occurred always with the greatest intensity. Thus the general tendency of the majority is for a greater intensity to require a lesser interval to produce the movement impression. Further, for the two subjects with qualitative results, with a given interval as the in-

tensity increases the impression passes from that of a single touch, through a row or motion to two discrete touches. Both these facts agree with those found by Korte.

V. *Distance and Intensity*

In Table IV. the distance between the stimuli and the intensity of the stimuli were the variables. The times of exposure and interval were constant, in most cases 60 and 30 sigma respectively. In a few series other times were used, as indicated. The successive columns indicate intensity of the stimuli in terms of the current in the solenoids. The rows for any subject indicate the distance between the stimuli. The most frequent report for each combination of distance and intensity is indicated as previously. In a given row the report which shows the most marked movement illusion is italicized. *F*'s results are given in the form of the previous tables, *i. e.*, the average number of touches felt per trial.

The array of italics for the various subjects shows a rather general tendency toward a distribution extending obliquely upward toward the right, *i. e.*, with a greater distance between stimuli a greater intensity affords a better illusion of movement. This too is in accord with the results of visual experiments. Further, for a given distance, as the intensity varies from weakest to strongest there is somewhat of a tendency for the effect to pass from unity through movement to discreteness, and for a given intensity increase of distance tends to produce increase of discreteness, a result similar to Korte's. In this latter connection we may note Benussi's finding that with distances greater than 16 cm. on the forearm there was an individualizing of the ends following the superficial movement.

VI. *Unequal Intensity*

In all the experiments up to this point the two successive stimuli on a given trial were equal in intensity. This factor was now varied as had been done in the visual and auditory experiments. With exposures of 60 sigma and interval of

TABLE IV

DISTANCE AND INTENSITY

(Exposures 60 sigma. Interval 30 sigma)

Subject	Distance	1.2 Amps.	1.4 Amps.	1.5 Amps.
A	12 cm.	Ends	*Flow or series*	*Flow or series*
	8 cm.	*Flow or series*	Series	Ends or series
C	12 cm.	One or both ends	*One end rolling*	Ends sometimes spread
	8 cm.	*Three rolling*	Three rolling or walk	Ends sometimes spread
D	12 cm.	Ends	*Ends or several simultaneous*	Ends
	8 cm.	One or snake	*Good snake*	Two or snake
F	12 cm.	4.4	5.0	3.8
	8 cm.	4.2	4.4	4.4

Subject	Distance	2.1 Amps.	2.3 Amps.	2.8 Amps.	3.6 Amps.	4.4 Amps.
A	16 cm.	Ends close		Ends		Ends
	12 cm.	Ends or flow or series		*Flow or series*		Ends
	8 cm.	*Ends or flow*		Ends		Ends
D	16 cm.	Ends or one		Ends		*Ends or series*
	12 cm.	One or series		*Series*		*Series*
	8 cm.	One or two		*Long series*		Series
C	16 cm.	Ends	Ends	Ends	*Ends with connection*	
	12 cm.	Two adjacent	*Two close, with loop*	Two at one end	Two adjacent	
	8 cm.	One or two	*3 or 4 successive*	One or two	One or two	

(Exposures 60 sigma. Interval 45 sigma)

Subject	Distance	2.1 Amps.	2.3 Amps.	2.8 Amps.	3.6 Amps.
C	16 cm.	Ends or one	Ends with line on skin	Ends with or without loop	*Ends with loop*
	12 cm.	One or two	One or two	*2 or 3 with loop*	Two connected or with loop
	8 cm.	One	*One or two with loop*	One or two	One

30 sigma, with stimuli 8 or 12 cm. apart and with the intensity of the first stimulus 1.5 amps., that of the second was increased from 0.5 to 2 amps. It was varied irregularly through various intensities and trials were intermixed in which both were equal.

Table V. gives the per cent. of trials in which the apparent movement occurred in the direction the reverse from that of the actual temporal succession. The first column gives the per cent. of trials in which the reversal effect occurred when the second stimulus was more intense (including all variations

TABLE V

UNEQUAL INTENSITIES

(Exposures 60 sigma. Interval 30 sigma. Distance 8 or 12 cm. Intensity 1.5 amps.)

	Per Cent. of Trials Yielding Reverse Movement	
Subject	Second Stronger	Equal Intensity
A	10.5	0
C	21.4	0
D	32.0	0
F	13.1	0

of intensity); the second column gives the same figures for the trials with equal intensity of the two stimuli. The results were obtained on several days and are based on some 50 trials of each sort for each subject.

It is evident that whereas with equal intensity of the stimuli the reversal effect never occurs, with the second stimulus more intense by various amounts the effect occurs in from 10 to 32 per cent. of the trials with the different subjects, an average of 19 per cent. This per cent. is much less than that found by the writer in the auditory illusions.

No attempt was made to correlate the results with the extent of the intensity difference or to study other aspects of the reversal effect (Korte's delta movement) as was done in the visual studies for the simple reason that the reversal effect occurred so much less frequently. However it is significant that the effect does exist there just as it does in vision and audition.

D. CONCLUSIONS

The above experiments investigated in the tactual illusion of movement the most salient of the factors noted by Korte in the similar visual illusion. The following are the principal results:

1. Two punctate tactual stimuli on the forearm for equal lengths of time separated by a discrete time interval and a

few centimeters apart, yield under certain conditions of time, distance and intensity an impression of movement from one point to the other in the direction of the actual temporal succession.

2. To produce the optimal impression of movement the following relations are required between the different variables under consideration:

(*a*) The relation between the length of exposure of the two stimuli and the time interval between them is not especially clear cut but on the whole is inverse—the longer the exposure the relatively shorter the interval.

(*b*) The greater the distance between the stimuli the greater the time interval.

(*c*) The greater the intensity of the stimuli the less the interval.

(*d*) The greater the distance between the stimuli the greater the intensity.

3. If the intensity of the second stimulus is greater than that of the first the illusory movement is sometimes produced in the reverse direction.

The above results are quite similar to those found by Korte in the visual illusion of movement. They are also the same as those found by the writer in a preliminary study of auditory illusions as far as that investigation was carried.

It seems a significant fact that the same illusion of movement is found in vision, audition and touch and that the same laws as to the relation between the variables involved hold to a considerable degree in the three fields. This points of course to a central origin. Whether it is due as Wertheimer claims to a physiological 'Kurzschluss' between cortical areas corresponding to the two stimuli at different points in space is an open question. Such a correlation between points of external space and points of the brain is perhaps plausible in vision and touch. The writer has pointed out[1] that such a correlation is highly improbable in audition and that hence a different explanation is necessary, and inasmuch as the three sense departments show the same phenomena it is better to adopt a single theory applicable to all three.

[1] *Op. cit.*, p. 75.

The writer has suggested (*op. cit.*) that the illusion is due to a continuity of motor impulse, and such a theory would be applicable to touch as well as to vision and audition. A tactual stimulus on the forearm produces an impulse to make some motor adjustment to bring that region into the focus of attention, presumably an impulse to turn the head or eyes toward that point. This would have, of course, a biological significance. A second stimulus, farther up the arm for example, gives a second impulse, presumably in the same centers of the motor cortex, to turn the head or eyes still farther. If the interval is of such length that the second impulse supervenes just when the first is about exhausted this continuity of motor impulses gives the movement illusion. When the second follows too quickly the two are not differentiated sufficiently and there is fusion and if the second is too late there is discontinuity and discreteness. Benussi found it impossible to obtain certain of the phenomena when the stimuli were applied to the forehead. This fact is suggestive inasmuch as the usual motor adjustments of the head to bring that region into the focus of attention are impossible.

The various relations between the different variables necessary to produce the optimal illusion are as explicable by such a theory as by the Kurzschluss theory. If we assume that intensity, distance and length of exposure are related positively to the temporal facilitation of the motor impulses and to their consequent sooner subsidence, all the above phenomena can be readily explained. The writer reiterates his suggestion that the action theory can account for the movement illusion which occurs similarly in vision, audition and touch.

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FACILITATION AND INHIBITION OF MOTOR IMPULSES

A Study in Simultaneous and Alternating Finger Movements

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Description of Experimentation

The purpose of the investigation[1] was to ascertain the facilitating and the inhibitory effect of successive and simultaneous muscular impulses in the movements of the several fingers of both the right and left hands. The data have also been arranged to show the effect of practise and fatigue and the relation of the movements of the fingers of the right hand to those of the left hand.

The instrument used was similar to that of the Whipple tapping-board[2] as will be seen from the cut, p. 476. In place of a metal stylus, a ring was worn on the finger. This was insulated by rubber tubing except on the under part where a metal peg protruded and made an electric contact with the metal base of the board. A light flexible wire ran from the ring to an electric marker which registered the contact on a kymograph.[3] In order to obtain a free and natural movement of the fingers, the hand rested on a curved block of wood, and was thus slightly vaulted. The tips of the fingers and cushions of the palm of the hand rested on the board. The arm rested on the table, several of the subjects using a cushion for greater comfort.

It was deemed of importance that the height of the finger

[1] The experiments were conducted in the Harvard Psychological Laboratory during the academic year, 1913–14. Trial experiments, not here reported, were made the previous year.

[2] 'Manual of Mental and Physical Tests, Simpler Processes,' p. 131.

[3] It was under similar conditions that one of the first tapping tests—that of von Kries—was performed, 'Zur Kenntniss der willkürlicher Muskelthätigkeit,' *Arch. f. Anatomie u. Physiologie*, 1886, Sup. Band, pp. 1–16.

movement should be constant so that there could be no doubt that differences in the rate of tapping were not due to changes in the length of stroke. A bar was therefore placed 4 cm. above the board and the subject was instructed to hit the bar at each stroke. The bar met the finger slightly below the second joint, and care was taken that it should always strike at approximately the same spot throughout the tests. This arrangement worked very well, the subject soon becoming used to the task. The movement thus remained more nearly voluntary than would probably have been the case if the finger had not had to touch the bar.[1] The hand was not strapped to the board as it was soon evident that the subject could keep his hand and arm still, and if there did happen to be a slight movement, his attention was called to it at once; nor was the tapping continued to that period of fatigue when the subject in seeking relief begins to use other muscle groups of the arm. Any slight error that might have crept in could not be as great as would have been the danger to the result caused by the binding of the muscles. The only muscles used, therefore, were the extensors and the flexors of the phalanges. These muscles pass from their origin in the forearm over the wrist joint to the phalanges and at the end of the grasping movement there is a tendency for them to flex the hand, which is inhibited by the synergic muscle of the wrist.

The time in two-second periods was marked on the drum at the beginning of each set of trials by a marker actuated

[1] In regard to the length of stroke, von Kries writes, *loc. cit.*, page 4, "Der Umfang dçr Bewegungen ist auf die Dauer von nur geringem Einfluss; doch scheint es, dass die Bewegungen von einem gewissen mittleren Umfamg am schnellsten ausgeführt werden können und sowohl die sehr kleinen als die sehr grossen ein wening länger dauern." Bryan ('On the Development of Voluntary Motor Ability,' *Amer. Jour. of Psychol.*, 5, 1892, p. 150) is in agreement with von Kries as to the slight effect of change of amplitude. Max Isserlin states that ". . . die Tendenz besteht, trotz abnehmender Geschwindigkeit die Bewegungszahl konstant zu erhalten. Diese wird zuletzt herabgesetzt" ('Ueber den Ablauf einfacher willkürlicher Bewegungen,' *Psych. Arbeiten*, 6 1910, p. 186). From this we may conclude that the change in amplitude conceals the fatigue as measured by the rate of tapping alone. It was also found in the preliminary tests before the control bar was used that there was a strong tendency for several subjects to execute a series of quick reflex movements similar to a tremble which greatly increased the tapping rate and seemed difficult at times to avoid.

by the laboratory clock. When both hands were used, a board was employed for each hand.

There were four subjects who will be referred to as *A*, *B*, *C*, *D*. *A* and *B* were advanced graduate students; *C* and *D* undergraduates. *A* and *D* were very athletic, *B* less so and *C* the least strong of the four. The experiments were made in the morning, the subjects coming always at the same hour. As a rule, there was a week's intermission between each set of trials. The period of tapping for all fingers and all combinations of movements was 30 seconds with a two seconds' pause between the members of a series and a five minutes' pause between the series. The finger movements examined were as follows: During the first half year, the subject began by tapping with his right index finger (*R*1). This was followed by the second finger of the same hand (*R*2). Then these two fingers were tapped alternately (*A*), a movement similar to the walking movement, that is, *R*1 was raised as *R*2 was lowered, the two fingers passing each other in the middle of the stroke. There then followed what may be termed complete alternation (*CA*). *R*1 made a complete stroke up and down before *R*2 began. The signal for a finger to begin was the return of the other finger to the starting point in the manner of a relay race. *R*1 went up and down then *R*2 went up and down, etc. Finally *R*1 and *R*2 tapped simultaneously [*S*(*R*1 *R*2)]. This completed the series. After a five minutes' pause, the series was repeated in the same order. On alternate weeks, the order of the series was reversed, beginning with *S* and ending with *R*1. At the beginning of the year the left-hand fingers were tapped in the same manner as the right and again for one series about six weeks later, in order to ascertain if there was any transfer of practise. During the second half of the year, both hands were used. The series began with the right index finger alone. Then followed the left index finger alone (*L*1) and then both simultaneously [*S*(*R*1 *L*1)]; and then the second finger of the right hand (*R*2) alone for thirty seconds, followed by the second finger of the left hand (*L*2) alone and then both simultaneously [*S*(*R*2 *L*2)]. This series was repeated and

the order reversed on alternate weeks. A few series were also made with $R1$ and $L2$ and $[S(R1\ L2)]$ and $R1$ and $L4$ and $[S\ (R1\ L4)]$.

Simultaneous Movement

The final averages have been gathered of the simultaneous tapping of the pairs of fingers of both hands and have been placed together in Table IV.[1] for convenience of comparison. In the second and fourth columns for each subject are the rates of tappings for 30″ for the fingers separately, and in the sixth column, the rates when tapped together. In the seventh column is the difference in rate of the two fingers, and in the eighth, the difference between the rate of the simultaneous tapping and the rate of the slower finger when tapping alone. Examining first the two fingers of the same hand, $R1$ and $R2$, we find the interesting fact that with all the subjects the two fingers are moved more rapidly together than the slower finger alone and in the case of *A* faster even than the faster finger by a considerable amount. With *B*, *S* is one stroke faster than the faster finger. A similar relation holds with the symmetrical fingers of the left hand (second horizontal column). Here in fact in the case of all but *B*, who shows little change, the two fingers are tapped faster together than either of the separate fingers. In Tables I., II. and III. the maximal rates of tapping are in heavy type and may be readily compared. For the right hand the highest *S* is greater than the highest $R1$ or $R2$ for all subjects but *C*. For the left hand the highest *S* is greater than the highest $L1$ or $L2$ for all the subjects. The explanation which suggests itself is that the two fingers being very closely related, an impulse to one tends to influence the other, since a strong coördination has probably been induced by the grasping movement. In the single movement the other finger must be voluntarily held down and this slows up the action of the moving finger. When both fingers are moved together this inhibition is removed and they both move faster, unless one is much slower than the other, when it acts as an inhibition. It is possible

[1] These averages have been taken from Tables I., II. and III.

that with the left hand, the inhibition of the idle finger is more difficult than with the right hand due to less practise. This would account for *S* being greater than the single tapping

Table I

No.	Subject *A*					Subject *B*					Subject *C*					Subject *D*				
	R_1	R_2	*A*	*CA*	*S*	R_1	R_2	*A*	*CA*	*S*	R_1	R_2	*A*	*CA*	*S*	R_1	R_2	*A*	*CA*	*S*
1	154	165	120	37	172		137	116	36	163	178	148	44	23	160	139	161	106	25	172
2	155	170	121	39	192	168	141	131	34	170	161	162		24	174	149	158	112	34	174
3	153	165	117	34	187	143	138	131	41	161	119	121	68	27	114	137	152	108	32	151
4	184	190	126	40	196	172	161	136	40	155	115	117		31	132	147	162	112	36	157
5	145	153	118	40	178	173	152	140	35	170	174	162	57	29	175	141	148	104	34	154
6	146	163	115	46	187	170	164	143	42	188	193	151	67	27	190	136	146	102	36	138
7	159	171	119	43	167	178	160	133	39	166	191	168	56	26	168	133	152	109	35	135
8	170	166	136	47	197	166	170	141	42	164	194	165	66	26	174	136	153	103	40	134
9	137	145	120	43	163	160	151	127	40	181	178	161	54	24	167	121	146	103	35	148
10	154	143	130	44	179	189	171	168	59		187	159	59	25	186		145	105	36	148
11	155	159	131	37	150	178	178	133	37	173						142	168	113	40	146
12	160	150	137	40	168	192	186	154	45	181						141	160	109	40	146
13	141	136	139	41	176	169	161	142	33	169						145	156	105	42	148
14	159	150	137	46	169	170	170	138	40	177						129	158	114	34	148
15	157	161	133	45	161	178	178	155	44	188						129	151	106	36	138
16	154	150	134	52	182	180	175	153	53	178						135	148	101	32	149
17						186	177	151	41	187						139	162	103	38	158
18						185	185	144	40	198						143	153	110	35	155
Av.	155	158	127	42	176	173	164	141	41	174	169	151	59	26	164	138	154	107	36	150
R1 to S	149	153	125	42	177	174	160	140	40	178	178	157	56	25	175	135	153	106	35	154
S to R1	161	164	129	42	176	173	168	142	42	170	159	142	63	27	147	138	156	108	36	144
m.v.	7	10	8	4	11	9	13	9	4	9	22	14	6	2	17	6	5	3	3	8

of either finger for the two subjects who did not show this with the right hand.

Is, however, the release of inhibition due to the close relationship of the fingers the only factor which causes the

Table II

No.	Subject *A*					Subject *B*					Subject *C*					Subject *D*				
	L_1	L_2	*A*	*CA*	*S*	L_1	L_2	*A*	*CA*	*S*	L_1	L_2	*A*	*CA*	*S*	L_1	L_2	*A*	*CA*	*S*
1	110	128	46	27	135	134	131	111	32	131	115	113	44	24	116	134	120	90	30	156
2	125	140	55	36	164	139	143	119	35	140	111	117	58	27	128	137	132	105	34	143
3	107	128	56	30	133	143	142	119	34	131	114	117	81	22	125	137	135	104	38	127
4	113	125	68	37	141	154	146	114	36	142	113	113	71	25	120	151	139	106	35	141
5	107	122	59	32	133	149	148	115	44	155	110	109	53	28	118	137	125	97	34	134
6	113	119	68	36	140	150	149	113	49	164	110	120	59	28	121	136	133	98	38	142
Av.	112	127	59	33	141	145	143	115	38	144	112	115	61	26	121	139	131	100	35	140
m.v.	5	5	6	3	7	6	4	2	5	10	2	3	10	2	3	4	5	5	2	7

increase especially in the slower finger? Does not one impulse directly influence the other when discharged simultaneously, not only exciting an inhibitory effect in the case of the slower movement, but a facilitating effect in the case of the faster? To answer this question symmetrical fingers of the two hands, *R*1 and *L*1 and *R*2 and *L*2 were tapped simultaneously. Here there cannot be the same strong natural tendency to move the two fingers simultaneously as is prob-

TABLE III

No.	Subject *A*						Subject *B*						Subject *C*						Subject *D*				
	*R*1	*L*1	*S*	*R*2	*L*2	*S*	*R*1	*L*1	*S*	*R*2	*L*2	*S*	*R*1	*L*1	*S*	*R*2	*L*2	*S*	*R*1	*L*1	*S*	*R*2	
1	146	117	137	147	124	155	172	146	155	170	143	163	179	107	118	168	114	133	130	124	128	151	1
2	157	119	150	151	123	164	184	153	167	177	151	174	191	114	127	167	116	122	135	125	128	151	1
3	164	128	140	145	117	156	173	144	163	174	148	151	190	113	122	171	124	139	131	128	127	152	1
4	159	115	149	168	140	158	193	146	169	180	148	154	200	116	123	180	114	126	**147**	140	133	**164**	1
5	135	129	147	156	131	151	174	150	165	177	148	165	173	123	131	169	117	135	133	132	131	153	1
6	160	131	146	173	**141**	152	180	158	162	174	154	161	192	111	124	**184**	124	**141**	138	136	130	158	1
7	169	117	152	170	131	155	192	**159**	175	185	154	159	180	115	123	169	114	120	142	132	132	153	1
8	**177**	130	144	160	132	150	**203**	153	167	**186**	**160**	170	172	116	123	120	101	134	142	**144**	136	153	1
9	167	133	**163**	163	125	150	180	151	157	186	149	156	182	123	**134**	169	125	134	138	132	128	157	1
10	162	**138**	160	158	132	**166**	187	148	157	172	146	153	**204**	119	122	162	111	118	138	129	130	153	1
11	158	119	143	167	123	151	193	153	170	180	148	162	176	121	120	156	118	124	135	140	133	154	1
12	162	125	150	**174**	131	155	200	148	**178**	185	152	**182**	179	**124**	132	164	**135**	133	143	133	**141**	163	1
13							175	145	162	173	152	165											
14							186	152	162	172	158	160											
Av.	160	125	143	161	129	155	185	150	164	178	151	162	185	117	125	165	118	130	138	133	131	155	1
R1 to S	155	129	140	158	131	156	179	150	161	175	150	162	187	116	126	170	118	130	135	129	129	154	1
S to R1	166	122	146	164	129	154	192	150	170	180	152	163	181	118	124	160	118	130	140	136	134	156	1
m.v.	7	6	7	8	5	4	8	4	5	5	4	6	9	4	4	10	6	7	4	5	3	3	

ably the case with the fingers just examined. In Table IV. we find that with three of the subjects for both sets of fingers there is an increase in the tapping of the slow and a decrease in that of the fast finger. In the case of both *A* and *B* the simultaneous tapping approaches the average of the two fingers. For *C* the effect of the faster finger is not so great and the increase of speed of the slower finger is below that of *A* and *B*. A further peculiarity of this subject to be discussed later offers an explanation for this. For *D* the simultaneous tapping for both pairs is about the same as the slower finger. The difference between *R*1 and *L*1, however, is very slight

and between $R2$ and $L2$ less than with any of the other subjects. This difference is an important factor as will be seen below (p. 460). It is evident, however, that with three of the subjects the rapidity of the movement is increased by the simultaneous exercise of a more rapid movement taking place in a symmetrical part of the opposite side of the body. The more rapid movement, on the other hand, is to an extent inhibited.

Will this phenomenon occur if the fingers moved are not symmetrical? To investigate this point $R1$ and $L2$, $R2$ and $L1$, $R1$ and $L4$ and $L1$ and $R4$ were tapped simultaneously.[1]

TABLE IV

Subject *A*								Subject *B*							
*R*1	155	*R*2	158	*S*	176	3	21	*R*1	173	*R*2	164	*S*	174	9	10
*L*1	112	*L*2	127	*S*	141	15	29	*L*1	145	*L*2	143	*S*	144	2	1
*R*1	160	*L*1	125	*S*	148	35	23	*R*1	185	*L*1	150	*S*	164	35	14
*R*2	161	*L*2	129	*S*	155	32	26	*R*2	178	*L*2	151	*S*	162	27	11
*R*1	161	*L*2	140	*S*	132	21	— 8	*R*1	181	*L*2	150	*S*	160	31	10
*R*2	157	*L*1	132	*S*	132	25	0	*R*2	174	*L*1	149	*S*	158	25	9
*R*1	160	*L*4	89	*S*	93	71	4	*R*1	177	*L*4	133	*S*	144	44	11
*R*4	107	*L*1	140	*S*	109	33	2	*R*4	145	*L*1	152	*S*	150	7	5
Subject *C*								Subject *D*							
*R*1	169	*R*2	151	*S*	164	18	13	*R*1	138	*R*2	154	*S*	150	16	12
*L*1	112	*L*2	115	*S*	121	3	9	*L*1	139	*L*2	131	*S*	140	8	9
*R*1	185	*L*1	117	*S*	125	68	8	*R*1	138	*L*1	133	*S*	131	5	—2
*R*2	165	*L*2	118	*S*	130	47	12	*R*2	155	*L*2	133	*S*	133	22	0
*R*1	193	*L*2	125	*S*	129	64	4	*R*1	135	*L*2	139	*S*	127	4	—8
*R*2	167	*L*1	126	*S*	120	41	— 6	*R*2	152	*L*1	139	*S*	138	13	—1
*R*1	195	*L*4	98	*S*	95	97	— 3	*R*1	141	*L*4	84	*S*	111	57	27
*R*4	113	*L*1	119	*S*	112	6	— 1	*R*4	98	*L*1	139	*S*	106	41	8

In the combination $R1$ $L2$ both B and C and in $R2$ $L1$, B still show an increase in the rate of the slower finger. A has now dropped below the single tapping for $R1$ $L2$ and D is below for both. With $R1$ $L4$ and $R4$ $L1$ all the subjects but C show an increase in the slower movement. C's simultaneous movement is slightly below that of the slower finger.

In order more readily to examine and analyze these results the difference between the tapping rates of the two fingers

[1] It has not been thought necessary to give a complete table of these tests. The averages were taken from fewer series than the previous ones, but as the general relationship does not vary materially from series to series they can be safely used.

has been placed in the seventh column of Table IV. and the increase in the rate of the slower finger during the *S* movement in the eighth column. A minus sign, of course, indicates a decrease.

Examining first *B*'s result we find that in the asymmetrical pair *R*1 *L*2 there is less of an increase of the slower movement than with the symmetrical pair *R*1 *L*1, an increase of ten as compared to fourteen, but the pair *R*1 *L*4 which is more asymmetrical than *R*1 *L*2 shows a slightly greater increase, *i. e.*, eleven compared to ten. In this latter case, however, the difference between the rates of the two fingers *R*1 and *L*4 is greater than between the former pair *R*1 *L*2. The former is raised one third of the difference, the latter only one fourth of the difference. The result suggests that there are two factors influencing the rapidity of the simultaneous movement, the degree of symmetry and the difference in the rapidity of the two members of the pair. These two factors should act in opposite directions; the difference between the rate of the two fingers increases as a rule with the decrease in symmetry, and the greater this difference in rate the more should the slower finger be aided by the faster in simultaneous tapping, but the greater the assymmetry the less is the advantage of simultaneous tapping. The relation of these two factors very likely differs in individuals. When the coördination is not good asymmetry probably plays an important rôle in slowing the movement. When the coördination is good the rate differences have more effect. Let us examine the data further with this suggestion in mind. Take for example *B*'s *R*1 *L*1 and *R*2 *L*2. The asymmetry is the same but the difference in rate of the *R*2 *L*2 pair is less than that of the *R*1 *L*1 and consequently the increase of the slower movement is less. With *R*1 *L*4 the asymetry is increased but the difference rate is also, so that the actual increase in rate remains the same as the other pair. In the case of *A* with the same two pairs it is true that the results are in the opposite direction, but in the next pair there is a drop in both symmetry and difference and there is in consequence, a falling off in the rate of simultaneous tapping in the one case even below the slower of the pair.

In the most asymmetrical pair, *R*1 *L*4, the difference is very large and there is again an increase of the slower movement notwithstanding the great asymmetry. In *R*4 *L*1, a pair of like asymmetry, there is less difference and less increase. Turning to *D*'s record, we find that although, like the other subject, he showed an increase when the two fingers were of the same hand whether the right or the left, as soon as the movements are on opposite sides there is often evidence of an inhibition. In the *R*1 *L*1 pair we should not expect much change for there is little difference between the rates, but with the *R*2 *L*2 pair, although the difference is twenty-two taps there is no increase of the slower finger, and in the *R*1 *L*2 pair the lack of coördination actually causes an inhibition of the slower movement amounting to eight taps. The results for *R*1 *L*4 and *R*4 *L*1 taken in connection with the foregoing results of this subject, speak strongly for the assumption of the above mentioned opposing factors. The pairs are the most asymmetrical but the differences in rate are very large, being for one almost three times as great as the largest previous difference. Examining the rate for simultaneous tapping we find that the slower movement increases by twenty-seven taps for *R*1 *L*4, that is, the facilitation is 31 per cent. of the rate when the finger is moved alone, and for *R*4 *L*1 there is an increase of only eight but the difference is less, forty-one compared to fifty-seven. The inhibitory effect of asymmetry which, judging from the previous results, is most probably present, has been more than overcome by the facilitating effect of the rate difference. This explanation also fits *C*'s result although he differs in type from the other subjects. It will be found when we come to the further test performed that *C* showed much more pronounced lack of coördination of different muscle groups than the other subjects. There should therefore be less facilitation and probably even inhibition. But he also showed the largest rate difference. Therefore, although the facilitation is less than in the case of *A* and *B*, nevertheless it is present in some instances. In the pair *R*1 *L*4 the large difference of ninety-seven was not sufficient to overcome the inhibition of asymmetry and when

we notice that with the much more symmetrical pair *R*1 *L*2 the large difference of sixty-four was only able to cause a facilitation of four taps, and with *R*2 *L*1 there was a difference of forty-one and yet a decrease of six taps, this result is rather to be expected. The only figures which do not readily offer themselves to the explanation here attempted are those of the pair *R*2 L2. The symmetry of *R*2 *L*2 is the same as *R*1 *L*1 but a difference of forty-seven in the former pair causes a facilitation of twelve taps while a difference of sixty-eight in the latter pair only causes a facilitation of eight. It is true that with none of the subjects is the relation of facilitation to rate difference constant. This ratio also varied with the different subjects. Two points might be mentioned in this regard. First, equality in symmetry does not necessarily mean the same amount of coördination of different muscle impulses. Thus in the results of *C*, although *R*1 *L*1 and *R*2 *L*2 are both symmetrical pairs the coördination between *R*2 *L*2 is better than between *R*1 *L*1, and perhaps for that reason the smaller rate difference has a greater facilitating effect. This explanation could also be offered in regard to the similar results of *A*. Secondly, although the change in symmetry between any two pairs is naturally the same for all subjects, yet one subject will probably have a different change in coördination in going from one pair to another, than a second subject, and this will readily explain individual differences in the above mentioned ratio. It should also be mentioned that investigations on other subjects revealed a difficulty to synchronize, which retarded the simultaneous movement.

Alternating Tapping

As was stated above the alternating tapping was performed in the same series as the simultaneous and the figures for *R*1 and *R*2 may again be used. *A* is the alternation in which one finger ascends while the other descends and *CA* the complete alternation in which one finger does not begin to move until the other has returned. The figures express only the rate of one finger, the number of actual taps made being twice that number. In Table V. these alternations are

expressed in per cent.. of the average of the two fingers tapping singly. Let us first discuss the *A* results. If the fingers were alternating levers of a mechanical machine there would, of course, be twice as much work done in the same time as one lever working alone would perform. In the human machine when two different movements, and in this case opposite movements, are carried on simultaneously we look for some

TABLE V

	Right Hand		Left Hand	
	$\frac{A}{\frac{R_1+R_2}{2}}$	$\frac{CA}{\frac{R_1+R_2}{2}}$	$\frac{A}{\frac{L_1+L_2}{2}}$	$\frac{CA}{\frac{L_1+L_2}{2}}$
Subject *A*....	.81	.27	.50	.28
Subject *B*....	.83	.24	.80	.26
Subject *C*....	.37	.16	.54	.23
Subject *D*....	.73	.25	.74	.26

inhibition. The amount of this inhibition is expressed in the per cent. It will be seen that for three of the subjects *A*, *B*, and *D*, with the right hand there is only a loss of about one quarter of the speed of one finger when working alone. In other words by carrying out simultaneously two movements, though opposite in nature, there is a gain of fifty per cent. as compared to the amount of work done if only one movement was performed in the same time. The other subject, *C*, shows a much lower figure. It is only thirty-seven per cent., which means that there is an actual loss in work accomplished by alternating simultaneous movements of thirteen per cent. In his present state of muscular coördination he would accomplish more with one finger moving alone than he would by moving two fingers. Fifty per cent. would mean that the work accomplished by the two fingers is the same as if one had moved alone. The actual figures make the above perhaps clearer. Alternating he only taps fifty-nine times for each finger, or one hundred and eighteen taps in all, while the mean of the rate of the two fingers tapping alone is 160. The loss is forty-two taps or twenty-six per cent. for the two fingers. This subject is the one referred to on page 461 as having poor coördination between different muscle groups.

The above figures make this evident. In this connection it is very interesting to note that *C*'s rate of tapping with one one finger is faster than any of the other subjects with the exception of *B*. From this it would seem that the inhibition is a variable independent of the rate of movement of the separate muscle groups. This assumption is strengthened by the results of the left hand. The separate tapping rates are much lower but the *A* rate is about the same as before, consequently the per cent. is higher. Subject *A* shows this same independence. The *A* rate for the left hand has dropped relatively much lower than the separate rates and the per cent. is consequently lower. In fact it is now about the same as *C*'s rate. That is, his coördination in the left hand is worse than in the right. Subjects *B* and *D* show almost identically the same amount of coördination for both hands. To repeat, the above results substantiate a fact which from what we already know is rather obvious, that the degree of coördination between several muscle groups does not bear any definite relation to the degree of efficiency of the separate muscle groups concerned.

Particularly in regard to the coördination of these movements it seems of interest to inquire into the musical training of the subjects. *A* is proficient with the violin, *B* has played the organ since boyhood, *C* has just begun to take piano lessons, and *D* has played the piano for years for his own amusement. *B*'s ratios of 83 per cent. and 80 per cent. are the highest of any subjects and one is disposed to say that this is due to greater practice and that *C*'s low ratio is due to lack of training. *A*'s figures are what one would expect. Being a violinist the fingers of the left hand are trained to a different set of movements. In playing the fingers are bent and one is held down while the other taps. There would therefore be an inhibition when the fingers were forced to tap alternatingly. This would account for the 50 per cent. which is even lower than that of the untrained *C*. These results, then, seem to indicate degrees of practice but they are too few to be more than a suggestion.[1]

[1] O. Raif argues that the fastest rate required by any piece of music is slower than the average rate of tapping, and therefore piano practice does not increase the rate

There remain to be examined the results of complete alternation (*CA*). Again using the illustration of a mechanical machine the one lever begins to move after the other has stopped. The work done in a given time is the same as if there were only one lever which moved continuously instead of alternating with the second lever. Each finger carrying out such a movement should do fifty per cent. of that which it would do if working continuously. Instead it will be seen that all the subjects with the exception of *C* do only about twenty-five per cent. whether with the right or left hand. *C*'s loss is again greater than that of the other subjects for the right hand. This difference is less than in the *A* rate and with the left hand he shows almost the same per cent. of loss as they do. These figures mean that in this alternation there is a fifty per cent. loss in muscular work done. This amount of loss seems to be relatively the same for both the right and left sides.

Index of Righthandedness

The indices of righthandedness of the first and second finger in the performance of the different combinations here investigated are given in Table VI. As has been done both by Woodworth and by Wells[1] the index is obtained by dividing the rate of tapping of the left hand by that of the right, thus giving the ratio of the efficiency of the two sides. As has been found in the tapping with the whole hand there are great individual differences. The range is even greater than that found by Wells. It is significant that subject *C*, who showed poor coördination in the more complex movements has also the lowest *L1*/*R1* index which means that he is also relatively inferior to the others in this simpler coördination for the left

but rather the proper timing of the movements. He says: "Nicht in der Bewegung an sich, sondern in der Rechtzeitigkeit der Bewegung, d. h. in dem Zeitverhaltniss von einer Bewegung zur anderen liegt die Schwierigkeit. Diese Rechtzeitikgeit kann zweifellos nur ein Product unseres Willens sein, wir haben also den Ausganspunkt für die Fingerfertigkeit in den Centraltheilen unseres Nervensystems zu suchen, etc." ('Ueber Fingerfertigkeit beim Clavierspiel,' *Zeitschrift für Psychol.*, 24, 1900, p. 354.) While upon the subject of characterization it is worth mentioning that *B* has the fastest tapping rate and *D* the slowest, and both are very athletic, as was mentioned above, which means that here there is zero correlation between strength and rate of tapping.

[1] 'Normal Performance in the Tapping Test,' *Am. Jour. of Psychol.*, 19, 1908, p. 446.

hand. The indices for the efficiency during the first and second half year's work have been given separately under each subject, and we notice that the change is not great. Of these indices five are slightly lower, two are the same, and one higher. That is, there is an indication that practice has had somewhat more effect upon the right hand than the left.[2] The individual characteristics, however, remain unaltered, the ranking of the subjects according to the size of index being the same. With three of the subjects the $L1/R1$ index is somewhat lower than the $L2/R2$ index due to the superiority of the efficiency of the index finger of the right hand. This is not the case with subject *D*, whose *R2* is throughout decidedly the most efficient finger.[1]

TABLE VI

Subject	$\frac{L1}{R1}$	$\frac{L2}{R2}$	$\frac{(L)A}{(R)A}$	$\frac{(L)CA}{(R)CA}$	Subject	$\frac{L1}{R1}$	$\frac{L2}{R2}$	$\frac{(L)A}{(R)A}$	$\frac{(L)CA}{(R)CA}$
A...	.72	.80	.46	.78	*C*.....	.66	.76	1.03	1.04
	.78	.80				.63	.71		
B...	.83	.87	.82	.92	*D*....	1.00	.85−	.93	.97
	.81	.85				.96	.86+		

In the *A* and *CA* movements subject *B*, who had the best coördination, shows an index similar to his index for simpler movements. Subject *A* has the same for *CA* but as shown above his coördinated wth the *A* movement on the left side was poor and his *A* index is therefore much lower than his other indices. Subject *C* has the same great difficulty with both hands and consequently has practically no index. Subject *D* has a higher index for the *CA* movements. From

[2] Wells's results are similar to these. He writes: "Again, in neither subject does the left hand show an improvement relative to the right. In Subject *I* the index of right-handedness remains practically the same. In Subject II. the right hand may even improve more than the left." *Loc. cit.*, p. 454. Our subject *A* with one of the fingers of his left hand showed an improvement relative to the right-hand finger. Whipple remarks, *loc. cit.*, p. 143, that "practice affects the left hand no more than the right; consequently the index of right-handedness is unaffected by repetition of the test." This generalization is not borne out by all of the subjects in this experiment, nor by all of Wells's subjects.

[1] The subject could give no reason for this. He had never to his recollection exercised this finger more than the others, and believes it must be an inate characteristic.

these results we again see the low correlation of these complex coördinations with the simpler coördinations of the single movement as was shown before.

Practice Effect

The experiments were not arranged with the idea of investigating practice and fatigue, but it does not seem amiss to discover what evidence there is of their effect under the conditions described. An examination of the tables I., II. and III. shows as was to be expected from the results of previous work that the improvement is not a steady one. If curves were plotted they would reveal the characteristic fluctuations. As stated, the maximum rate for each series is in heavy type. See Tables I., II. and III. It will be seen that it may occur at almost any point of the series, nor is the *R*1 maximum necessarily obtained on the day of the *R*2 maximum or the *S* maximum on the day of the maximum of either finger concerned, nor does the *A* maximum always occur on the day of the *AC* maximum. In short there is a low correlation as regards the days of the maximum results for the different fingers of the different movements.

Table VII. has been arranged to show the general change between the first and second half-year's work, and between the first and second part of the first half year. The figures in the first and second horizontal columns are the averages for the first and second half of the series given in Tables I. and II. The third, fourth, and fifth horizontal columns contain the result of the second half year's work.

It will be seen that with the separate tapping of the fingers of the right hand in the case of two of the subjects, *B* and *C*, there is decided evidence of the effect of practice, not so much with *B* in the difference between the first and second half year as in the difference of the halves of the first half year. The other two subjects do not show this difference, in fact *A* in the first half year shows a falling off. With the left hand fingers *B* and *C* again show the effect of practice, but it is not so marked as with the right hand. Subject *A* shows a practice improvement in the index finger of the left hand,

and subject *D*'s results again show no evidence of practice effect. In the alternating movements *B* shows decided improvement as does also *A*, while *C*, who had great difficulty, shows a considerable loss. *D* remains about the same. In the complete alternation there is no significant changes. The change in the simultaneous movement follows the change in the individual movement.

The most evident fact in these results is the wide individual differences which preclude any general statement. It is

TABLE VII

Subject	*R1*	*R2*	*A*	*CA*	*S*	*L1*	*L2*
A.......	158	168	121	41	184	112	127
	152	149	133	43	168	125	129
	160	161					
	160						
	161	157					
B.......	169	154	136	41	168	145	143
	180	176	148	41	181	150	151
	185	178					
	177						
	181	174					
C.......	157	143	68	27	158	112	115
	187	163	59	25	174	117	118
	185	165					
	193						
	195	167					
D.......	140	159	106	34	152	139	131
	136	155	107	37	148	133	133
	138	155					
	141						
	135	152					

interesting to note, however, that practice does affect the result in some cases even though the daily amount of work of each finger is slight and there is a week's interval, if not more, between each series.[1] Another fact to be noticed is that the practice gain in the more complex coördination, as in the case of the *A* movement, may be independent of the progress of a less complicated movement. In the case of subject *A* both *R*1 and *R*2 show a loss in rate in the second half of the series, *R*2's loss being considerable, and yet the *A* movement,

[1] The difference of practice gain between the two hands appeared in the difference of indices for right-handedness, p. 466.

which is a coördination of these two, shows a decided gain. Subject *C* shows a gain by practice in *R*1 and *R*2, and a loss in the *A* movement. Finally the fact that the more voluntary movement, *CA*, shows practically no change in rate, should be emphasized.

The two horizontal columns next to the last column in Tables I. and III. are arranged to compare the averages of the two orders of procedure. The first one of these horizontal columns for each subject gives the averages when a single finger movement precedes the double, simultaneous movement, the second when the reverse order is used. In both series the rate for the *R* finger for all subjects except *C* and one figure for *B* is more rapid when the *R* succeeds than when it precedes the *S* movement. In the majority of cases the difference is as large or larger than the m.v. Only in two instances is the rate for the *L* appreciably affected by the order and the results are of opposite nature. The simultaneous movement in three cases is decidedly more rapid when it starts the series, in two instances the difference is greater than the m. v. The only results, then, that seem at all significant are those that show the *R* movements more rapid when they succeed the other movements. The possible explanation is that with this finger the warming up effect was greater than the fatigue. It must be remembered that there was a pause of two minutes between the 30-second tests which could very well be sufficient for recovery of this finger but not for the others. In the wrist-tapping test Wells used two and a half minute pauses and his results show an increase in rate as the series progressed.

Fatigue

The amount of fatigue in each 30-second series is calculated by finding the relation of the difference in rate of the first and second 15 seconds to the entire 30 seconds. These were found for the results in Tables I. and III. and are given in decimal form in table VIII. The absolute differences are also given. For instance, the first figure in column 3, *i. e.*, .047, was found by dividing the rate of *R*1 which is 155 into 7.3 which is the

difference of the two 15-second halves.[1] The averages for all the subjects are given in column 10.

Most striking is the fact that fatigue is greatest for the *A* movement and that there is no fatigue in the *CA* movement. It must be remembered that the *A* movement is the rapid antagonistic movement, the *CA* movement is very slow and one set of muscles rests while the other reacts. This readily explains

TABLE VIII

	Subject *A*		Subject *B*		Subject *C*		Subject *D*		Av.
*R*1	7.3	.047	6	.034	12	.071	6	.044	.049
*R*2	6.5	.041	4.5	.028	8	.053	9	.059	.045
A	9	.071	7	.05	4	.07	6	.056	.062
CA	—1/8		—1/18		—2/5		—1/3		
S	7.5	.042	5	.028	5.5	.033	6	.04	.036
*R*1	4	.025	5	.027	11	.059	6	.043	.039
*L*1	5.5	.044	4	.027	5	.043	7	.053	.042
S	2.5	.017	5.5	.034	4.5	.036	6	.046	.033
*R*2	5	.031	5	.028	7	.042	10	.065	.041
*L*2	5	.039	5	.033	2	.017	6	.045	.033
S	6	.039	5	.031	6	.046	3.5	.026	.035

the absence of fatigue in 30 seconds. This is not only true for the averages but with few exceptions for all the subjects. For subject *C* the *A* movement fatigue is about the same as the *R*1 fatigue. The *S* movement for *R*1 and *R*2 for two subjects is the least fatigueable, for the other two it is the same as the lowest index of the single finger. In the *S* movement for *R*1 and *L*1 and *R*2 and *L*2 the index is the lowest in three instances and only twice is it higher than both of the single movement indices.

It may be said, therefore, that in general the simultaneous movement of two fingers for 30 seconds does not show more, and very often less fatigue than one finger. A comparison of *R*1 and *R*2 in the two halves of the table shows as other experiments have before, that practice has the tendency to reduce the fatigue. Only in one instance out of eight is it

[1] In view of the manner in which the results were recorded, it was thought better to divide the periods in halves rather than to compare the first five seconds with the averages of the 1st, 2d, 3d, 4th, 5th and 6th five second periods as Wells did. *Loc. cit.*, p. 469. His index is higher probably because the initial spurt has thus more influence on the result, it being reduced in the above index by the results of the 2d and 3d five-second periods.

greater with practice. As has frequently been found the decrease of fatigue is the important factor in practice gain. No correlation can be found between the fatigability of the two hands. Nor can a general statement be made as to the fatigue index of the right as compared to the left hand. In three subjects the relation between $R2$ and $L2$ and $R1$ and $L1$ in regard to fatigue is in the same direction, except that subject *B* shows the same fatigue for $R1$ and $L1$ while $R2$ is less than $L2$, but subject *D* shows opposite results. For him the fatigue for $L1$ is greater than for $R1$ and for $L2$ less than $R2$. One cannot say, therefore, that if the $R1$ finger is more easily fatigued than the $L1$ finger, that the $R2$ finger will be more easily fatigued than the $L2$ finger.

Variations.

There is less variation in the left-hand movements than in those of the right hand. This is what both Bryan[1] and Wells[2] found for wrist movements. The small m. v.'s accompany the slower reactions but there are even indications of a less relative variation on the left side. The simultaneous movement shows about the same variations as the single movements. The *CA* movement shows the least variation of all the movements. This movement being very slow (fewer taps per 30″) the relative variability is higher than with the other movements. No general statement can be made in regard to the *A*-movement. There is a tendency for it to be relatively larger than that of the single movement. Absolutely it is sometimes larger and sometimes smaller.

Summary and Conclusion

If the index and second finger of the right hand are tapped simultaneously as rapidly as possible the resulting rate according to the results of four subjects of varying degrees of motor ability, is faster than the rate of the slower finger and may even be more rapid than the faster finger when tapping singly. There is doubtless a more or less innate coördination

[1] *Loc. cit.*, p. 163.
[2] *Loc. cit.*, p. 480.

between the movements of the fingers of the same hand caused by the biologically important grasping reflex. When the extensor of the index finger is innervated a tendency for the symmetrical extensor of the next finger to move is also observed.[1] Ths impulse must be inhibited and it is probable that this inhibition also extended to the motor half-center of the first finger causing a loss in rate of movement. An inhibition somewhat similar in nature has been demonstrated by Sherrington in his experiments on the stepping reflex when he simultaneously stimulated two afferents which are antagonistic in their effect. He says: "Of the two afferents concurrently stimulated, that one which when stimulated alone causes flexion of the joint excites the flexor half-center and inhibits the extensor half-center; and the other afferent, which when stimulated alone causes extension of the joint, excites the extensor half-center and inhibits that of the flexor. When both afferents are stimulated simultaneously with appropriate intensity, the discharge from the flexor half-center represents the algebraic sum of the opposed excitation and inhibition which the two afferents individually exert on it, etc."[2] In our experiment it is the inhibition of one flexor half-center which is communicated to the other flexor half-center and this inhibition is then compounded algebraically to the excitation of the extensor half-center. When both fingers move simultaneously this inhibition is removed. This at least seems to be a plausible explanation of the fact that the simultaneous movements were faster than either single movement. Whether it was the only factor could readily be tested by tapping simultaneously symmetrical fingers on two hands. It was found that under these conditions the movement of the slower finger was facilitated, the degree varying with different individuals. The two fingers together, however, never tapped faster than the faster finger.

[1] L. Huismans states "Homolaterale M–B. (Mit-Bewegungen) sind in den weitaus meisten Fällen auf eine Irradiation des Bewegungsimpulses in der Hirnrinde Zuruckzuführen" ('Über Mitbewegungen,' *Deutsche Zeitschrift für Nervenheilkunde*, 40, 1910, p. 233).

[2] 'Reflex Inhibition as a Factor in the Coördination of Movement and Postures,' *Quarterly Journal of Experimental Physiology*, 6, 1913, page 269.

Numerous examples are to be found in the literature relative to the influence of the movement of one side of the body upon that of the other. In the simple reaction experiment Paul Salow found that the reaction time for simultaneous movements of two hands was longer for each hand respectively than when they reacted separately.[1] M. L. Patrizi found in his ergograph tests that in simultaneous action both hands did less, but when they worked alternately the right hand action reinforced that of the left hand. He believes that one cannot give maximal attention to the two simultaneous acts.[2] W. W. Davis found that in general the right hand tapped more rapidly alone than in connection with either the other members. Two of his subjects were able to tap more rapidly when all four members were tapping. He remarks that "with a longer practice the right hand, in multiple tapping, would undoubtedy excel in rapidity its record while tapping alone." He also concludes from his results that "there is a close connection between different parts of the muscular system through nervous means. This connection is closer between parts related in function or position." In this work Davis was not interested in the influence of the faster on the slower member.[3] Mention should also be made of the fact that a paralyzed member may be moved by moving a healthy member.[4]

[1] 'Untersuchungen zur uni- und bilateralen Reaktion. II. Einige Versuche am Chronographen,' Psychol. Stud., 8, 1913, pp. 506–540).

[2] Patrizi writes: "Les recherches que j'ai rapportées dans ce mémoire nous font admettre une uncompatibilité d'états psychiques, même quand il s'agit de la coïncidence, dans le même instant, d'impulsions maximales destinées à des mouvements symétriques et homogènes et qui sont habituellement accouplés." In regard to Féré's results, which indicate that when the left hand is almost fatigued its capacity can be increased by the movement of the right hand, he says that an increase will not occur if the two movements are exactly simultaneous ('La simultanéité et la succession des impulsions volontaires symétriques,' *Arch. Italiennes des Biol.*, 19, 1893, p. 138).

[3] 'Researches in Cross-Education,' Yale Studies, 6, 1900, pp. 6–50.

[4] See J. Grasset, 'L'action motrice bilatérale de chaque hémisphere cérébral,' *L'annee Psychol.*, II., 1904, pp. 434–445. It is interesting to note that W. P. Lombard said some years ago: "Not enough work has been done to admit definite statements concerning the strengthening or weakening effect of the action of the one hand upon the other. . . . The few observations which have been made with reference to this question favor the idea that if one hand acts simultaneously with the other it tends to weaken rather than strengthen its movements. This effect is not a constant one, however, as

In searching for an explanation for this contralateral facilitation of the slower movements by the faster, the fact of sympathetic movement seems the most significant. Everyone has had the experience when the member which one desires to move is held or when it has become fatigued, of moving the opposite member. In this regard Ch. Féré remarks that the examples drawn from his research "indiquent que, lorsqu'il existe un obstacle à un mouvement volontaire unilatéral, l'influx nerveux a une grande tendance à prendre la voie symétrique du côté opposé." This tendency is greater in children, being later more or less suppressed.[1] In the simultaneous movement of the two fingers the faster finger must be held back to synchronize with the slower. According to the above if the one finger were not already moving while the other finger was being held back from its full movement the sympathetic movement of the former would probably be evident. When it is moving at the same time as the finger which is being somewhat retarded, its movement is facilitated by the surplus energy of the faster finger. There is also another explanation or, perhaps, a second factor in conjunction with the above and that is the increased amount of peripheral stimulation, *i. e.*, the contact with the board and bar. We know from the work of Sherrington, Alexander Forbes, T. Graham Brown, and others that the afferent impulse on one side of the body may cause a contralateral reflex. Now the afferent path on the ipsolateral side may be fatigued and the stimulus on the contralateral side may become more effective. Alexander Forbes says: "The fact that central fatigue induced through one afferent nerve usually does not impair the reflex involving the same muscles induced through another afferent nerve supports the conclusions of Lee and Everingham that this fatigue does not involve the moto-neurones, and accords with the view of Sherrington that its seat is the synapse."[2] This explanation

many exceptions occur" ('Alterations in the Strength which occur During Fatiguing Voluntary Muscular Work,' *Jour. of Physiol.*, 14, 1893, p. 116).

[1] L'alternance de l'activité des deux hémisphères cérébraux,' *L'Année Psychol.*, 8, 1901, p. 148.

[2] 'The Place of Incidence of Reflex Fatigue,' *American Journal of Physiology*, 31, 1912–13, p. 122.

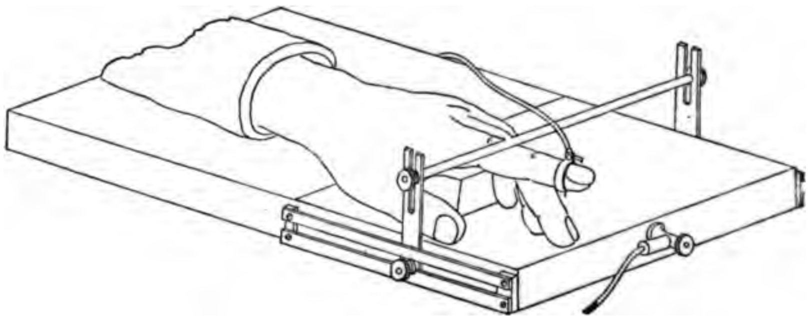
can also be applied to simultaneous tapping on the same hand. Owing to the fact, however, that these movements were never carried on sufficiently long for any great fatigue this factor must play, if at all, a very small rôle.

Thus far we have been discussing simultaneous movements of symmetrical members. This facilitation was also observed between asymmetrical members but it was not so great. There can be little doubt that the coördination is less perfect the more asymmetrical the members are. Even the transfer of practice is greater to symmetrical parts.[1] There is not, however, a very high degree of correlation between increasing symmetry and increasing facilitation with all the subjects. In order to explain the results an antagonistic factor has been suggested, *i. e.*, that, ceteris paribus, the greater the difference within limits between the rates of the two fingers the greater the facilitation of the slower. This difference increases with the asymmetry and at times causes an asymmetrical pair to show greater facilitation than a more symmetrical pair. An explanation which includes two opposing factors can explain anything and should not be used without strong proof. There are, in this series of experiments, instances where the degree of symmetry is a constant and we see here the effect of difference in rate in the direction just mentioned. We have also cases of similar degrees of difference in rate and here the effect of symmetry can be seen. The results are not sufficiently consistent to be conclusive. They are, however, suggestive and the explanation founded upon them must necessarily also bear that adjective.

2. An examination was also made of the simultaneous movement of the index and first finger of the right and left hand when these movements were in opposite directions, a combination of movements similar to the stepping reflex and spoken of in this paper as alternating movements. Here there is double reciprocal innovation according to Sherrington. The four subjects were divided into two groups in respect to the amount of coördination. With three of the subjects each finger was able to do about three quarters as much in this

[1] See W. W. Davis, *loc. cit.*, page 49.

combination as when tapping alone, except for one of the subjects with the left hand. The loss for each finger due to this combination of impulses was only 25 per cent., so that in the same time the pair was able to do one and a half times as much as a single finger. The fourth subject showed a much poorer state of coördination. Two of the three subjects with good coördination were piano players and the other who had a low efficiency with the left hand had his fingers of that hand trained for another system of coördination, for violin practice. Judging, however, from the great differences be-



tween these subjects and from observations on others, it seems probable that this test shows some fundamental differences in motor coördination. It would appear profitable to gather further data with special reference to the effect of practice, the tests here described being too few to draw any conclusion from them on this point.[1]

Another fact which appears clearly in the results is that the amount of coördination between the two reflexes does not run parallel with the development of these reflexes. The subject who has the worst coördination has one of the highest

[1] Sherrington, in speaking of grace in walking, says that "the proper execution of the act ensures a moment of complete rest to each of the opposed motor centers engaged." *Loc. cit.*, p. 271. T. Graham Brown found very decided differences in the reflexes of the cat " . . . these individual variations are probably due in great part to more fundamental differences in the constitution of the nervous centers. Some cats are 'walkers.' They exhibit in a marked degree the phenomena of 'narcosis progression.' Other cats are 'scratchers.' In them the scratch-reflex is peculiarly excitable" ('Studies in the Physiology of the Nervous System, XIV. Immediate and Successive Effects of Compound Stimulation in Spinal Preparations,' *Quarterly Journal of Experimental Physiology*, 7, 1914, p. 200).

rates in single tapping. He is a subject with little piano practice, so that this ability in single tapping substantiates Raif's assumption (vide supra, p. 464) that piano players do not show any special ability in rate of tapping.

3. The term complete alternation has been applied here to successive movements of the two fingers; one finger completes both the extensor and flexor movements before the other begins. With all the subjects and with both right and left hand the two fingers in this combination tap only one quarter the amount that they do in the same time in tapping continuously and alone. The loss in this form of alternating movement is about 50 per cent. This combination is a system of successive reflexes. The loss in efficiency is probably in great part due to the necessary inhibition of the previous reflex. As Sherrington says ". . . there will persist during the new reflex activities belonging to the old with, in result, confusion of the two. Rarely, indeed, can it happen normally that the reflex machinery in executing a train of different reflexes is actuated by a train of different stimuli, each one of which abruptly ceases just as the next one begins."[1] And further: "For orderly and unconfused sequence of reflex acts—also of willed acts—central inhibition is a necessary element of coördination in the transition from one muscular act to another."[2] Another reason for the loss is that in the single tapping movement inhibition of one of the antagonistic movements increases the tendency for that reflex to discharge, causing what might be termed a rebound. If, however, the finger must rest after the completion of each flexor movement until the other finger has completed its movement, most of this post-inhibitory effect is lost.[3]

4. Even with an interval of one week between the tests a practice gain in most of the movements is noticeable. An exception must be made with the most voluntary movement, namely, the complete alternation. The subject who had the worst coördination in the alternating movement even showed a loss. The practice effect in the alternating movement does

[1] *Loc. cit.*, p. 275.
[2] *Loc. cit.*, p. 276.
[3] See Sherrington, *loc. cit.*, p. 278.

not run parallel to the practice effect of the fingers tapping separately. A gain in the former may be accompanied by a loss in the latter. Practice in the simultaneous movement, on the other hand, does follow that of each finger when working separately. There is some evidence that practice affects the right hand more than the left.

5. Fatigue is noticeable for all the movements except the complete alternation.[1] It is greatest for the alternating movement and generally least for the simultaneous movement. That is, during thirty seconds there is less fatigue when two fingers are working simultaneously than when one is working alone.

6. The index of righthandedness is not necessarily the same for all the movements, nor is it always the same between the different pairs of symmetrical fingers.

7. The variations in tapping rate are less for the left hand.

[1] Wells says: "To sum up, the maximum rate of repeated voluntary movements is a function that practically every investigator working with sufficiently accurate methods has found to be subject to fatigue effects, though the degree of this subjection has differed considerably" ('A Neglected Measure of Fatigue,' *Am. Jour. of Psychol.*, 19, 1908, pp. 352–3.)

CONCERNING THE IMAGE

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CONCERNING THE IMAGE[1]

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A few years ago Professor Winch introduced the subject of imagery with a fable about foxes, some of whom had tails and some of whom had not.[2] It was soon evident that those who had been deprived of their brush were meant to symbolize the happy possessors of an imageless mechanism. Considering the manner in which the image has been hunted down recently the simile is more apt perhaps than he realized at the time. Notwithstanding the attack which has been made upon the image I am going to assume that it still exists and shall devote my attention to describing what I believe to be some of its characteristic functioning, hoping that such a course will do more eventually to convince its enemies of its existence than theoretical discussion or introspective accounts of its texture.

In using the term image I do not wish to denote a content of consciousness qualitively different from sensation. Recent experimental work seems sufficiently convincing upon that point. I am referring under that name to sensations centrally aroused.

The meaning of an object is our attitude toward that object, our reaction to it. In like manner the meaning of an image is that inevitable change in our organism which follows its appearance in consciousness. This change may consist of immediate motor discharge and accompanying kinæsthetic sensations or in visceral, glandular or similar reactions characteristic of emotional states. In terms of the self we may say that we assume a definite attitude toward the image

[1] This paper was read at the meeting of the American Psychological Association, on December 28, 1915.

[2] 'The Function of Images,' *J. of Phil.*, 1908, 5, 337–352.

just as we do toward the object. The image acts for or represents the object and in doing so acts as a cue for a definite reaction belonging to the object. Of all possible purposes of an image certainly that of representing a situation not present to the senses is the most obvious.[1]

This function of representation is not, however, characteristic alone of imagery, but belongs also to sensations aroused by an external stimulus, and cannot therefore be used to distinguish it from the latter. The following illustration makes this point evident. If one comes suddenly upon a white sheet hanging upon a line in the moonlight one will probably inhibit one's mechanism for advancing and at least prepare for retreat. This will be readily recognized as a ghost reaction. The situation means ghost. It may be that our sensations will be supplemented by imagery and that we shall actually perceive a ghost. I am inclined to believe that in many instances if not always the imagery which completes the perception accompanies or follows the reaction rather than precedes it and that the externally produced sensation is the sole cue. When we think ghost it may happen that we shall have not a complete image of a ghost but a fragmentary one of the white sheet. Here the image alone is the cue and as in the case of the perception the reaction, to a large extent emotional, is the ghost meaning.

I have purposely chosen as an illustration an incomplete representation, *i. e.*, the white sheet for the whole ghost. The image may be an almost perfect likeness of an object. In most cases, however, it is not; and often it is so far removed from the original object that to say that it means the object

[1] Colvin says in 'The Learning Process,' p. 111: "... the meaning of a situation is after all an attitude, and that attitude must in the last analysis be a motor affair. Thus it is but a step to the conclusion that this general dependence of experience for its significance on motor adjustments had left a deposit or 'mind-stuff' that symbolically represents concrete situations, not actually present, but ideally represented."

Hollingworth writes: "Their place (objects of perception) is taken by any content, revived, perseverative, or immediately sensory in character, which happens to be easily available at the time. These processes, by a sort of substitutional rôle, come to represent and impersonate the object between which the relations, as socially recognized, would be said to hold" ('Vicarious Functioning of Irrelevant Imagery,' *J. of Phil.*, 1911, 8, 691).

seems on the face of it almost absurd.[1] But from what we know of the laws of association one thing is certain, namely that a hundred different images can introduce almost the same reaction, and if the reaction is the meaning then a hundred different images can in this sense stand for the same meaning, although ninety-nine of them must of necessity be what I should like to call inadequate images, if I may borrow an adjective used in the case of stimuli arousing paradoxical sensations.

Several years ago I obtained introspective accounts of imagery occurring in experiments on inhibition and hitherto unpublished.[2] As the introspection seems to me especially interesting among other things for the clear evidence of the motor attitude which attaches itself to what without due consideration of this setting would appear totally irrelevant imagery, I should like to introduce it here.

A method of experimentation was desired by which the subject would be required to suppress some very strong association. With that in mind the alphabet and the numerical series were selected. There were five subjects, all students of the Harvard Psychological Laboratory. Each was asked to repeat the alphabet as rapidly as possible and the time was taken with a stopwatch. Then he was asked to repeat the alphabet again omitting the letters f, l, r and x. The signal to begin was given about two seconds after the instructions and the time to repeat the alphabet again taken. This test was given in connection with other experiments, so that only one test was made each week, the series extending through about two months. The next week the subject was

[1] I am thinking particularly of the classic example of the image for 'but' described by Professor Titchener. The presence of inadequate imagery has been repeatedly mentioned in the literature. In the *Philosophical Review*, 1897, p. 650, J. R. Angell writes: "They (images) often shift, and are by no means the same at different times. Indeed, there is no necessity that they should be, representing as they do simply a medium for the performance of a certain function, *i. e.*, the function of meaning a definite thing. Thus the image which serves me in using the concept 'justice' is sometimes a visual image of a state of justice and sometimes simply the auditory word image." ('Thought and Imagery.')

[2] A brief account is given in the report of the meeting of Experimental Psychologists at Clark University, Psychol. Bull., 1912, 9, 236.

asked to omit g, m, s, and y. Similar arrangements were tried for several weeks. Then he was asked to repeat the numbers up to thirty omitting 5, 10, 15, 20, and 25. Then omitting 4, 9, 16, 19 and 26 and so on. Toward the end he was asked to omit the even numbers and finally to repeat the numbers in reverse order omitting 27, 18, 12, 9 and 4. Introspection upon the fore period (V.P.) and the main period (M.P.) was asked for at the end of each test.

From the full introspection I have selected the portion essential to an understanding of the imagery and manner of inhibition of each subject. Arranged according to the subject it is as follows:

Subject *A*

1. V.P. Auditory kinæsthetic, 'Omit f, l, r, x.' M.P. When I came to the letter to be omitted I made a short pause. Visual image of 'f.' Felt an inhibition.

2. V.P. There was a visual image of the letters and auditory image that I must not say them.

3. (As the experiment progressed we find) the fact that I was to omit the letters was not strongly represented in consciousness, but the letters themselves were much more prominent (in the V.P.).

4. V.P. Before I started I went through (skipped) from 5 to 7. 6 came up visually. (He was to omit 6, 11, 17, etc.) There was no tendency to say it.

5. (Finally we have): M.P. When I came to the one to be omitted, it came up as visual and auditory image, but no kinæsthesis. The inhibition was so conscious (powerful) that there was no attempt to say the number to be omitted.

Subject *B*

1. V.P. There was the visual imagery of the number scheme. I thought that when I came to them I must be careful. M.P. When I came to 4 I knew that I must stop.

2. V.P. I suppressed the numbers as I went along. I saw where the letters were. M.P. When I came to the letter before the one to be omitted I got excited. The letter came kinæsthetically. I felt as if I drew back.

3. V.P. There was the feeling of location of the numbers in a scheme. I felt keyed up when I thought of them. There was mild fear when I saw them.

4. V.P. Visual image of the numbers. I thought of the numbers before the one to be omitted as the ones to stop at. Feeling in throat and tongue. M.P. When I came to the one before the letter to be omitted I recognized the feeling and stopped.

5. V.P. I made myself see the number preceding each one. I tightened up my mouth and shoulders. Rigid feeling.

Subject *C*

1. V.P. I tried to think where the break was. I saw the letters as a series of steps with the letters to be omitted cut out. M.P. As I got to the letter there was a visual image which I had to throw off.

2. V.P. I thought of the letters as holes or notches that had to be jumped over. M.P. They came up kinæsthetically. As they came up I saw the breaks. This shut off the letters.

3. V.P. The letters were represented by hurdles to be leaped over.

4. M.P. Hurdles repeated. I did not see the numbers at all. They came up kinæsthetically and I had to inhibit them. All the time I was conscious of the field of hurdles.

5. V.P. The series was represented by a broken chain. M.P. I saw the break when I started. The number came kinæsthetically. I forced it back according to the visual image. That is what seemed to help me inhibit the number. Later the inhibition took care of itself.

Subject *D*

1. V.P. Visual image of immense gaps to be leaped. M.P. When I reached the letters to be omitted there was an auditory image of the letter and a clenching and stopping. A feeling went down as far as the diaphragm.

2. V.P. Visual image of the five letters to be omitted on a card. I knew I should have to stop at f. (g was to be omitted.) Nothing for the other letters. I trusted to luck for them.

3. V.P. Visual image of a stony field. The stones were numbered with the letters to be omitted. They were as obstacles. M.P. The numbers did not come into consciousness. I just stopped for some reason at the number before, and groped about.

4. V.P. Visual image of a football game. The numbers were represented by men whom I was to pass. M.P. I ran with the ball. The man who reminded me of six threw himself forward. I did not say six.

5. V.P. A visual image of a piano key-board. My idea to omit the numbers was to leave out the black keys while playing.

Subject *E*

1. V.P. I went over 5, 10, 15, 20, 25 to myself. I noted that they were at certain stages. I went from 4 to 6. Instruction in mind. M.P. I grouped them in fives, getting the first group from the V.P. As I went on it became easier and easier.

2. V.P. I repeated the numbers five or six times. I do not know how the instruction was present, but I knew that they were not to be said. After memorizing them I was conscious of the instruction to omit them. When I came to 3 (4 was to be omitted) I stopped as if I could not tell what to do. The instruction came into consciousness and I went to 5.

3. V.P. I repeated the numbers to myself and put my fingers down as I said them. I did not think what had to be done to them.

4. V.P. I thought of the numbers preceding the ones to be omitted. When I thought of 19 (18 was to be omitted into the backward series) the instruction was in mind. M.P. When I came to 28 the number to be omitted (27) was in mind, until I started to say 28. It then dropped out and I went over to 26, without 27 coming up again. At about 21 number 18 (the one to be omitted) came up and remained until I began to say 19.[1]

[1] For the most part the introspection, at times inexact in expression, is in the form in which it was given to the examiner. Some of the abbreviated sentences have been completed by me. The subject was not pressed for a more detailed description when he said he 'thought' or such and such was in mind. The experimenter was not at the time interested in the question of imageless thought.

It will be seen that visual imagery was used by four of the subjects and probably by the fifth. In general the method of inhibition was to associate in the V.P. some form of inhibition with the numbers either visually or verbally represented. Sometimes there was a direct association of the inhibition of the vocal organs with the numbers. Sometimes there was an association of the words of the instruction as intermediary. In one instance the fingers were pressed down as the numbers were spoken in the V.P. In another an emotion of fear was associated with the letters. The imagery at times was fantastic, such as notches or hurdles to be passed over or football players to be evaded, but inadequate as they may be they were in all instances of such a nature as to be readily associated with inhibitory processes. They mean 'inhibition' in that they function as such. It will be readily admitted that the perception of the same word can give rise to different meanings according to the setting. In like manner a visual image such as that of a hurdle, which was most strongly associated in the subject's mind with evasion, can in the above situation cause an evasion of a word, that is the inhibition of the vocal organs rather than the more direct association of the lifting of the feet.

In the main period the visual or auditory imagery frequently appeared as a cue and the inhibitory process followed. At times there was probably a direct effect of inhibition from one muscle group to the other. We know that such radiation of motor impulses occurs. The clenching of the fists and the pressing down of the fingers when other muscles are to be inhibited are examples. I feel rather convinced that in the case of inadequate imagery also there is such an action from one muscle center to another, the imagery directly initiating the motor response most readily aroused.[1]

[1] In this connection Wilfrid Lay's remarks ('Mental Imagery,' *Psychol. Monog.*, 1897–99, 2 (3), p. 29) are of interest: "If we attempt to visualize such lines as

'To take up arms against a sea of troubles
And by opposing, end them,'

the result is often the purest visual nonsense." It would seem from what I have said above that the imagery would probably not be nonsense, but a definite motor setting, a brace against an onslaught such as is necessary against the breakers.

The full introspection shows that there is a dropping off of imagery as the tests progressed and even in a single trial the imagery appears only for the first letter or number. The determining tendency—whatever that may eventually turn out to be—grows stronger and reaction follows reaction without the intervention of imagery.[1]

When there was difficulty the image tended to appear again. A good example is that from Subject *B*'s introspection, at the end of the series. He decided not to think of the numbers in the V.P. All went well for three numbers. He failed, however, to omit the fourth and the visual image of the fifth promptly appeared. This reappearance of imagery to help in time of trouble has frequently been observed in investigations on the learning process.[2]

It is undoubtedly because the task set in the above experiments was a difficult one that so much imagery appears so clearly described in the introspection and for that reason the test recommends itself for class-room practice.

The reactions in the above experiments are comparatively simple ones. There is merely an inhibition of the muscles of speech involved. It may be stretching the term to speak of an inhibition as a reaction, but there will be no confusion if

[1] See J. R. Angell, 'Methods for the Determination of Mental Imagery,' *Psychol. Monog.* 13, (1), p. 70 for a good description of the decrease in visual imagery with practice.

[2] D. F. MacLennan writes ('The Image and the Idea,' Psychol. Rev., 1902, p. 74): "Another proof of the close dependence of meaning upon imagery is found in the constant resort to imagery when thought is baffled. . . . The moment, however, that some new thought or some new combination of thoughts arises, we search for the concrete imagery in which the conception may be appropriately embodied."

M. R. Fernald ('The Diagnosis of Mental Imagery,' *Psychol. Monog.* 14 (1), p. 137) writes: "That we have had imagery reported for each of the tasks by each of the subjects may be seen by reference to our experimental data. The certainty and invariability of this response increase with the novelty or difficulty of the problem."

Similarly Book says in a footnote ('The Psychology of Skill,' p. 42): "It is psychologically important that all the images which appeared in the different stages of the writing were first prominent and distinct, then hazy and indistinct, disappearing entirely soon after they had served their purpose in the learning."

See also G. H. Betts, 'The Distribution and Functions of Mental Imagery,' *Teachers College, Columbia University Contributions to Education*, 1909, p. 94. He finds that imagery appears when thought is baffled but thinks that it is "for the most part irrelevant, and hence of no possible service in reaching the solution."

we speak of it as a motor setting. In the more intricate meanings we must look for a more highly organized response and this we find in the integrated action about which recent physiology has told us much.[1] And further there is no reason why a fragment of a most simple image may not be the cue for a very complex and intricate response.[2] A mere ghost of an image may serve as a cue for an extremely involved ghost response. This is particularly true of concepts, for a detailed image would have the tendency to inaugurate a dated and placed response, which is contrary to the purpose of concepts. This has seemed to me the explanation of the fact sometimes noticed that imagery of concepts is likely to differ somewhat from reality. I remember in experiments I made a few years ago they were invariably larger than reality; but that is only one of many possible forms of variation.[3]

A type of reaction to imagery which should be more generally emphasized in the literature is the emotional reaction. We found in the above introspection that the emotion of fear was directly attached to the image. This, of course, frequently occurs and leads most readily to a motor response. There is, however, a large field where the motor response as a practical attitude is absent by reason of the very nature of the attitude. I refer to the field of esthetics. Here imagery is very prominent, especially visual imagery, and the response is generally a subtle emotion with its accompanying bodily changes and empathic motor setting. The prominence of imagery in esthetic enjoyment where there is this non-practical attitude suggests a rather obvious explanation why in general comparatively little imagery is noticed and also why, when introspection discloses an army of clear and vivid visual impressions, many of us doubt the reliability of the introspection. The greater part of our attitudes toward our in-

[1] See E. B. Holt, 'Response and Cognition,' *J. of Phil.*, 1915, 12, 399–400.

[2] J. R. Angell (*loc. cit.*, p. 651): "In so far as an image is simply a symbol of certain experiences, great dimness in it is still compatible with its correct use within limits."

[3] In regard to the function of the image as cue and the importance of the motor setting in general ideas see Münsterberg's 'Psychology General and Applied,' p. 173.

E. B. Titchener, however, states: "I doubt whether particularity or abstractness of meaning has anything essentially to do with the degree of definiteness of my images." 'Experimental Psychology of the Thought Processes,' p. 16.

vironment are practical attitudes. Our interests are directed toward what we can do with an object and how we respond to it.[1] The objects themselves, from the fact that they are mere cues, remain for the most part in the margin of attention while the reactions occupy the fovea. It is the dreamer, the non-practical man, the man out of touch with his surroundings, for whom visions occupy the center of attention. This difference in attention is greater even when not in the presence of the object, that is when imagery is functioning in its stead. Then the slight imagery present in the extreme margin of consciousness of the trained mind is often as little noticed as the *mouches volantes* before the eyes.[2] To ask a subject to introspect upon his thought process must almost inevitably distort to some degree the picture, by bringing the imagery into too much prominence. Yet an argument against its too great prominence is not one against its existence.

I have spoken of the consciousness of the mature mind. From what we have said of the use of imagery in learning, in acquiring specific adjustments, it would follow that imagery is more prominent in childhood and this is what is generally found. I venture the suggestion that there is also much imagery in old age, but for quite another reason. In the late period of life action is of little importance and contemplation fills the soul. Witness the delight in minute and often tiresome description of trivial details.

In conclusion I would say that I thoroughly agree with the Behaviorists that our efforts should be directed with all possible energy toward the refinement of the methods for the investigation of expression. The science owes them the deepest gratitude for their reaction against objectively uncontrolled experimentation. As a psychologist, however, I cannot be satisfied with half the picture nor can I persuade myself that images which I have experienced do not exist, although they, together with sensations, may eventually be further analysed.

[1] See Judd's 'Psychology,' p. 238.

[2] In the classic words of Galton ('Human Faculty,' p. 112), "That it must afford immense help in some professions stands to reason, but in ordinary social life the possession of a high visualizing power, as of a high verbal memory, may pass quite unobserved."

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PORTABLE TACHISTOSCOPE AND MEMORY APPARATUS[1]

BY W. F. DEARBORN AND H. S. LANGFELD

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The front of the tachistoscope is a brass surface $9\frac{1}{4} \times 4\frac{1}{8}$ inches with a window $2\frac{1}{4} \times \frac{3}{8}$ inches. The window is closed by a paper shutter, which is worked by a brass spring. The speed is below $\frac{1}{10}$ of a second, that is, faster than eye-movements and can be easily regulated by tightening the spring. The shutter mechanism is behind the brass front and entirely concealed from the subject. By pressing the button at *A* the subject can operate the shutter himself or the experimenter can operate it by a string from behind. The brass front is on hinges and can be opened to insert the list of words. A small number is placed to the right of each word in the list and this number can be seen through the small hole *B* at the right of the shutter. It is thus known when the word is behind the shutter and what it is. By leaving sufficient space between the words and indicating the space position by dots on the margin (see sample list, p. 386) the shutter can be closed without exposing the word.

When the window is closed the paper shutter, by sliding between the brass front and a metal contact, opens an electric circuit. The contact is made when the shutter falls and the word is exposed. The exact time of the contact can be regulated by altering the height of the paper shutter.

By half cocking the shutter the window can be kept open. The angle of the brass front to the base can be altered to suit the convenience of the subject and the illumination. When the instrument is not in use the front can be closed, and it then fits into the base so that one has a closed box only an inch in thickness, which can easily be carried in the pocket. The instrument weights 17 oz.

[1] The instrument can be obtained from A. G. Cox, the mechanic of the Harvard psychological laboratory, Emerson Hall, Cambridge, Mass.

There is an adjustable brass pointer at *C* which may be used for the purpose of fixation and to direct the attention to the beginning, ending, or various parts of the words.

The instrument meets most of the requirements of an ideal tachistoscope: (1) There is no appreciable change of accommodation, as the point of initial fixation is separated from the word only by the thickness of a thin library card

Portable Tachistoscope and Memory Apparatus.

(paper shutter), a negligible quantity from the ordinary reading distance. (2) The tendency of the eye to follow the moving shutter, which is the disadvantage in most gravity tachistoscopes, is minimized in that the shutter and background are of the same color, and this tendency may be further decreased by enamelling the front surface of the instrument the same shade as the shutter. If this is done the

conditions of ordinary reading of black print on a white surface are more nearly duplicated. (3) The movement of the shutter is sufficiently noiseless not to be disturbing. (4) By reducing the instrument to a very simple form of construction it has been made sufficiently inexpensive to allow its use in quantities in training courses. (5) There is an advantage in the observer being able to release the shutter, as he can thus concentrate his attention on the window at the right moment.

The following are a few suggestions for experimentation, especially in training-courses:

1. As indicated in the sample list, the visual span for objects, numbers, letters, familiar words of various lengths, unfamiliar words, and short sentences may be investigated.

2. Observations may be made in regard to the reading by whole words or parts of words. For this purpose in addition to the unfamiliar words, mutilated words such as 'phylosophical,' and skeleton words such as 'ab-r-vi-t-d' may be used. An example of the reading by parts is as follows:

1st	reading	Cyt
2d	"	Cytore
3d	"	l ticum
4th	"	Cytorel
5th	"	Cytore
6th	"	culum
7th	"	ticulum
8th	"	reticulum
9th	"	Cyto
10th	"	Cytoreticulum

3. In order to investigate differences in the form of words, lists of words composed mainly of words with characteristic or determining letters or letter complexes and others composed of the more indifferent letters such as the letters of the line may be used. 'Alphabetical' and 'statistical' are examples of the former, 'conscience' and 'caravan' of the latter.

4. The suggested classification of readers into objective and subjective types may be tested, *e. g.*, by lists of words easily misread. The objective type is supposed to include

readers who have a narrow span but accurate perception, the subjective a wider span but a more suggestive perception. The subjective are supposed to depend in their perceptions more upon the total visual form, the objective more upon the determining or dominant letters.

5. As a test of range of information lists of words from various fields of knowledge may be used.

6. In order to test the influence of a definite mental set two lists may be used, the one composed of relatively unrelated words, the other of words from a specia field.

7. For memory experiments the shutter, as mentioned above, is placed at half cock, which leaves the window open. The lists of words are drawn past the window at a rate regulated by a metronome or sounder in circuit with a clock. The back of the instrument is so constructed that a belt of paper may be used instead of strips, thus obviating loss of time between the successive exposures of the list.

8. The electric contact permits the instrument to be used in reaction experiments by placing the contact in series with battery, chronoscope, and voice key.

N. B. If the instrument is manipulated entirely by the observer, lists may be placed in the instrument with a blank sheet over them and the blank sheet slipped out before starting. This is a useful method in case small classes of pupils in schools are to be tested.

Sample List

* * * *	1
* * * * *	2
* * * * * *	3
* * * * * * *	4
8197	5
68352	6
827153	7
1852379	8

bdcv	9
zjflmt	10
trpbhcd	11
kvnsqgtc	12
watch	13
books	14
wires	15
maple	16
run away	17
how do you do	18
practical	19
multiplication	20
statistical	21
alphabetically	22
caravan	23
environment	24
loquaciousness	25
micromaniac	26
succinamic	27
expansile	28

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PORTABLE SELF-REGISTERING TAPPING-BOARD AND COUNTER[1]

BY HERBERT SIDNEY LANGFELD

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The instrument consists of a small tapping-board with a brass tapping surface 5 × 3 inches (*A*), a stylus (*B*), and a box 5½ × 4½ × 3¾ inches (*C*) containing a Veeder counter, magnet, and a five-cell Eveready battery. The board is hinged to the box and can be folded up and hooked (at *D*) to the front of the latter, thus making the instrument very

compact, and convenient for carrying. The board is connected to one of the poles of the magnet through the hinge, and the stylus to the other pole. The magnet, which works the counter by means of a curved armature fastened directly to the shaft of the counter, is quick in action and the fastest

[1] The instrument can be obtained from Mr. A. G. Cox, the mechanic of the Harvard psychological laboratory, Emerson Hall, Cambridge, Mass.

tapping can be recorded accurately. The number of taps are read off on the Veeder through a small window in the top of the box at *E*. With this instrument, tests for muscular coördination and fatigue can readily be made in the schools and factories as well as in the laboratory and the results recorded with a minimum expenditure of the experimenter's time.

By substituting a finger ring for the stylus the instrument can be used in finger movement experiments similar to those described by the writer.[1] By a simple adjustment it can be employed to record the free finger movements recommended by Professor Raymond Dodge.[2]

Instead of the tapping stylus a contact pencil (*F*) can be connected to the magnet. This has a metal point (*G*) which, when slightly pressed down, closes a contact in the pencil and actuates the counter. With this pencil, curves, words of a manuscript, etc., can be counted accurately and without mental fatigue.

[1] 'Facilitation and Inhibition of Motor Impulses,' THE PSYCHOLOGICAL REVIEW, Nov., 1915, **22**, 6.

[2] 'Psychological Effects of Alcohol.' Washington: Carnegie Institution of Washington, 1915, p. 167.

THE DIFFERENTIAL SPATIAL LIMEN FOR FINGER SPAN

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THE DIFFERENTIAL SPATIAL LIMEN FOR FINGER SPAN

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The investigation of the limen for finger span was suggested by the practical demand for accuracy in estimating the number of objects or size of objects grasped between the thumb and index finger. The problem of psychophysical measurements in general, however, although one of the oldest, has rather increased than diminished in theoretical interest and importance, as the many functions involved, and at first so little suspected, have come to light, thus casting suspicion upon the accuracy of threshold measurements. In representing these results, therefore, the theoretical question of method and the desire to present further data for discussion are uppermost.

Beside the establishment of the limen, the experiment was arranged to ascertain individual differences, differences according to the method of presentation of the stimuli, the relation of the subjective feeling of certainty to the objective accuracy, and the value of the doubtful or guess judgments.

There were six subjects—five men and one woman. Three of them, *A*, *B*, and *F*, were trained experimentalists, of which *B* was the best trained. The other subjects had little knowledge in methods. Subjects *A* and *B* were slow, deliberate, and careful. The others were quicker and of a more nervous temperament. Subject *E* was particularly high strung and nervous to the extent of bodily twitchings. This description was corroborated by another experimenter who had all but one of these subjects.

The instruments used were six calipers, two of which were Starrett's No. 25 M as shown in the cut, page 419. The other four were modelled from them. Small, accurately machined right-angle plates with perpendicular surfaces,

16 mm. by 25.5 mm., were securely screwed on the ends of the two points (*A*). At the end of the adjustable arm rest were upright blocks which held the clips (*B*) into which the calipers could be easily slipped. The arm rest was adjusted for each subject so that when the hand rested comfortably on the board with fingers extended over the edge, the caliper plates could be grasped in their middle without moving the hand.

The procedure adopted was the method of constant stimuli. Not only did this method seem less open to theoretical criticism, but it was more practical for this experiment than the method of limits, since it would have been difficult to obtain an instrument that could be quickly adjusted with the necessary degree of accuracy. The standard distance was 5 cm., a distance arbitrarily selected as not being too great to be comfortably spanned by all the subjects and yet sufficiently large that relatively large differences could be used. The comparison stimulus was set at a distance of 1 mm. Five comparison stimuli were used, the two extremes being eliminated.[1] There were, then, two standard stimuli of 5 cm. each and four comparison stimuli of 5.2 cm., 5.1 cm., 4.9 cm., and 4.8 cm. respectively.

A long series of about seven hundred judgments for each subject was run through during the first half year and the effect of practice was thus in great part eliminated.[2] The results have not been included in this paper, and the method employed, which differed from that finally adopted, need not be described here.

The main experiment followed three weeks after this practice series (an unfortunate break due to the mid-year period). Three separate limens were obtained: one for the span of the index finger and thumb of the right hand, the standard and comparison stimuli being presented successively

[1] See Fernberger, S. W., On the Elimination of the Two Extreme Intensities of the Comparison Stimuli in the Method of Constant Stimuli, PSYCHOL. REV., 1914, 21, 335–355.

[2] Fernberger, S. W., The Effects of Practice in its Initial Stages in Lifted Weight Experiments and its Bearing Upon Anthropometric Measurements, *Amer. J. of Psychol.*, 1916, 27, 261–272.

to the same hand; another for the two hands, the stimuli being presented simultaneously to the two hands, the standard to one and the comparison to the other; a third, also for the two hands, but with the stimuli presented successively, first the standard to the one hand followed by the comparison to the other, and vice versa.

The subjects worked in pairs, alternating after each set of thirty judgments in order to prevent fatigue, and only ninety judgments were made in the hour. A week intervened between each group of ninety judgments. Each set of thirty judgments consisted of ten judgments for each of the three limens. The subject made ten judgments for the limen of the right hand, then ten when the stimuli were presented simultaneously to the two hands, and finally ten when the stimuli were presented successively to the two hands. On half the days this order was reversed, the stimuli being presented first to the two hands successively, and last to the right hand alone. The subject then read while the other subject went through the thirty judgments. The series of ninety judgments for the two subjects lasted about an hour. The entire experiment extended through the four months of the second semester.

The order of the presentation of the comparison stimuli was 5.2 cm., 4.9 cm., 5 cm., 5.1 cm., and 4.8 cm., with the comparison stimuli presented first in the successive presentation and to the right hand when two hands were used. This same order for the comparison stimuli was then repeated with the standard first and to the left hand when two hands were used, the time and space error being thus objectively eliminated. The judgment was given in terms of the second stimulus when successive and in terms of the right hand stimulus when simultaneous. The stimuli were applied at intervals of a second.

The subject sat at a convenient height in front of the table with his hand lying comfortably upon the rest, which was properly adjusted so that the fingers could grasp the plates at their centers. All the calipers were so set that the two plates were equidistant from the middle point of the

instrument. In thus keeping the center of the span always in the same place, there was no possibility of detecting a difference through a change in the symmetrical relation of the two plates to the hand. The subject closed the second, third, and fourth fingers and extended the thumb and index fingers, which were held a convenient distance apart. He closed his eyes, the experimenter placed the calipers and clips, and said 'Now.' The subject then closed his fingers on the two plates and gave his judgment. He was never at any time given a clue as to the accuracy of the results.

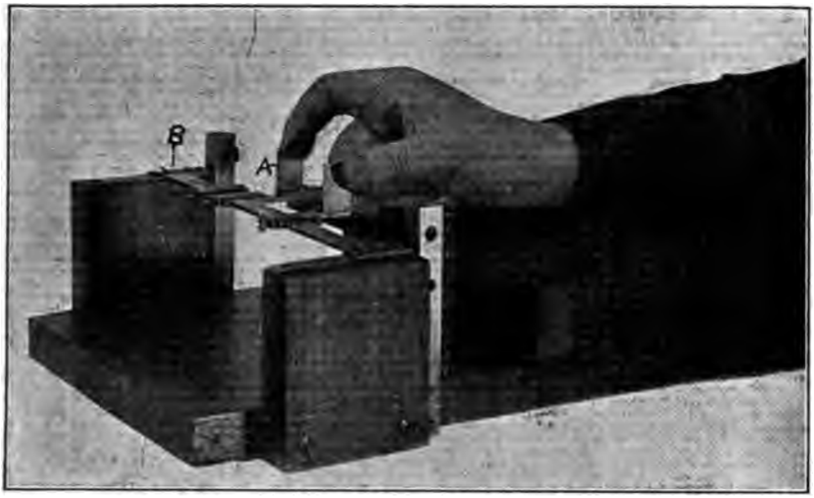

FIG. 1. Arm Rest with Caliper and Hand in Position.

The subject was instructed to clasp the plate with sufficient pressure to be able to feel the spatial extent separating them, the sensations involved being principally those of joint and tendon. He was particularly cautioned not to use the distance moved through by the fingers in clasping as a criterion. Special attention was paid to practice in abstracting from this movement perception during the five months of preliminary training, and from the introspection of all the subjects, it is fair to assume that there was the proper direction of attention and that the judgment was based on the static spatial perception.[1] The subjects were told to judge

[1] Inasmuch as the fingers did not always assume the same position previous to the grasping, this criterion of movement would have been a very uncertain one.

whether the second space was larger, smaller, or equal to the first, or in case of simultaneous presentation, whether the right was larger, smaller, or equal to the left. He was further told that there were objectively equal stimuli. He was directed to say equal only when there was an actual perception of equality, and not when there was a mere perception of no difference. He was directed to grade his judgments *a*, *b*, and *c*, according to his degree of confidence in his answer, *a* being the highest degree. If he could not make a larger, smaller, or equal judgment even with *c* confidence, he was to say 'equal *d*,' and was then to guess either larger or smaller. Under this category would fall all doubtful judgments, no difference judgments, and or-judgments.[1] Both judgments were noted for separate treatment. The response under these instructions, for example, took the form of 'larger *c*,' 'smaller *a*,' 'larger *b*,' etc. None of the subjects had the least difficulty in grading their confidence, the confidence response coming immediately and without hesitation after the first judgment. Even during the first series, no one complained of it interfering with his judgment, and as it did not take an appreciably longer time, it was decided to continue it throughout the series even though correlations of degree of confidence with accuracy of results and an analysis of the guess judgment had been made by previous investigators.

The method of treatment of the results was according to the $\varphi(\gamma)$ hypothesis.[2] The upper and lower limens, S_2 and S_1, the interval of uncertainty, $S_2 - S_1$, the Measurement of Sensitivity or Threshold of Volkmann, $(s_2 - s_1)/2$, subjective equality, $(c_2 + c_1)/(h_2 + h_1)$, and coefficients of precision, h_1 and h_2, were calculated. The results are recorded in Table I.

Directing our attention first to the M. S.[3] for the one hand,

[1] S. S. George believes that doubtful and or-judgments probably belong to the same category. For an excellent evaluation of these various judgments and an explanation of their non-serial nature, see his article entitled Attitude in Relation to the Psychophysical Judgment, *Amer. J. of Psychol.*, 1917, **28**, 1–37.

[2] See F. M. Urban, The Method of Constant Stimuli and Its Generalization, Psychol. Rev., 1910, *17*, 229–259. Also, S. W. Fernberger, On the Relation of the Methods of Just Perceptible Differences and Constant Stimuli, Psychol. Monog., 1913, No. 61, especially pp. 29 and 38. I am greatly indebted to Professor Fernberger for his valuable advice in regard to the mathematical treatment.

[3] 'Measurement of Sensitivity.'

it will be noticed that there is a considerable difference in the individual M. S. as well as in the coefficients of precision. The two subjects who have the highest coefficients of precision, subjects *A* and *B*, are two of the three best trained

TABLE I

Method	Subjects	h_1	h_2	$S_1=\left(\frac{C_1}{h_1}\right)$	$S_2=\left(\frac{C_2}{h_2}\right)$	S_2-S_1	$\frac{S_2-S_1}{2}$	$\frac{C_2+C_1}{h_2+h_1}$
Right hand	*A*.......	6.1604	5.6587	4.976	5.066	0.09	0.045	5.019
	B.......	5.520	5.042	4.94	5.03	0.09	0.045	4.987
		(4.9632)	(4.849)	(4.917)	(5.049)	(0.132)	(0.066)	(4.982)
	C.......	2.5922	3.7562	4.977	5.075	0.098	0.049	5.035
	D.......	3.5630	3.3988	5.009	5.074	0.065	0.032	5.044
	E.......	3.1709	2.9872	4.978	5.042	0.064	0.032	5.009
	F.......	(4.2877)	(4.5568)	(4.98)	(5.056)	(0.076)	(0.038)	(5.019)
Simultaneous	*A*.......	3.5461	3.978	4.982	5.081	0.099	0.049	5.035
	B.......	3.1252	2.809	4.822	5.097	0.275	0.137	4.952
		(2.9785)	(2.4428)	(4.772)	(5.135)	(0.363)	(0.181)	(4.935)
	C.......	1.3948	2.5185	4.966	5.002	0.036	0.018	4.989
	D.......	2.0402	2.5045	4.929	5.15	0.221	0.11	5.051
	F.......	3.9459	4.2674	5.025	5.035	0.01	0.005	5.03
	F.......	(1.6789)	(2.868)	(4.736)	(5.096)	(0.36)	(0.18)	(4.963)
Successive	*A*.......	4.0334	4.7442	4.97	5.087	0.117	0.058	5.033
	B.......	3.1591	2.412	4.894	5.102	0.208	0.104	4.984
		(2.4178)	(2.1034)	(4.821)	(5.155)	(0.334)	(0.167)	(4.977)
	C.......	2.5538	3.3941	4.933	5.047	0.113	0.056	5.002
	D.......	1.7494	2.6402	5.007	5.137	0.131	0.065	5.085
	E.......	2.6394	3.0165	5.02	5.068	0.048	0.024	5.046
	F.......	(2.0141)	(2.7383)	(4.951)	(5.058)	(0.107)	(0.053)	(5.012)

h_1 = Coefficient of precision "less judgments,"
h_2 = Coefficient of precision "greater judgments,"
S_1 = Lower limen,
S_2 = Upper limen,
$S_2 - S_1$ = Interval of uncertainty,
$\frac{S_2 - S_1}{2}$ = Measurement of sensitivity (Threshold of Volkmann),
$\frac{C_2 + C_1}{h_2 + h_1}$ = Point of subjective equality.

subjects, and the two who were the most careful and consistent in their attitude toward the experiment. Even in the other two methods, subject *A*'s coefficients are the highest, and *B*'s coefficients rank very high. Although some of the other subjects had lower M. S.'s, it seems preferable to consider the M. S.'s of these two subjects, which are identical, as the M. S. under the most constant conditions, rather than to average all the subjects. The data from the other subjects have been included, however, for comparison. The

M. S. is, then, 0.45 mm., which bears a ratio to the standard of about 1/100.[1]

The data of subject *F* have unfortunately little value and have therefore been bracketed. This subject and subject *B* had a great many doubtful judgments. For subject *B* there are available a sufficient number of judgments beyond the fifty required to permit the elimination of these doubtful judgments entirely and to substitute other judgments in their place. This was impossible in the case of subject *F*, so that the data include the doubtful judgments as equal. The data were calculated for subject *B*, both with and without the doubtful judgments included, the calculations for the former being included (bracketed) for comparison. It will be seen that with the doubtful judgments eliminated, the M. S. is lower and the precision higher. An explanation for this is given on page 427 where the doubtful judgments receive further treatment. Another factor that makes subject *F*'s results questionable is that during work in another university he had acquired a set not to give equal judgments and it was rather difficult for him to change this.[2] That may be a reason why he had so many doubtful judgments. His data are only used in discussing relative values. The other five subjects had very few doubtful judgments, so few that it was not necessary to eliminate them, as they made no appreciable difference in the results. The exact number of doubtful judgments for the subjects in all three methods, that is in seven hundred and fifty judgments, is as follows: subject *A*, 3; *B*, 100; *C*, 10; *D*, 14; *E*, 6; and *F*, 86.

The results of the subjective equality increase the confidence in the method. In all three methods of presentation and for all subjects, it approaches closely to objective equality. When we compare the results of the three methods of presentation, we find little consistency among subjects. From experience we would have prophesied higher M. S.'s and less

[1] Jastrow some years ago made some experiments upon finger span with wooden blocks of various sizes. He found that with a difference of 1/100 the subject made 3.5 errors in ten judgments. The Perception of Space by Disparate Senses, *Mind*, 1886, p. 552.

[2] See S. W. Fernberger, The Effect of the Attitude of the Subject upon the Measure of Sensitivity, *Amer. J. of Psychol.*, 1914, 25, 538–543.

precision for two hands. For precision this is the case, with the exception of subject *E* for simultaneous presentation. As regards the M. S., the subjects vary among themselves, even the two whom we have selected as most reliable. Subject *A*'s M. S. for the right hand is the lowest, but it is only slightly lower than his simultaneous, and not much lower than his successive M. S. Subject *B*'s M. S.'s, on the other hand, are much higher for simultaneous and successive presentations. Four subjects' M. S.'s are lower for the right hand, and two for simultaneous presentation. Some are lower for simultaneous than for successive presentation, and others the reverse. There are so many possible factors involved that it is impossible from the data at hand to offer satisfactory explanations. There is no doubt, especially with untrained subjects, that various criteria were used. Judging from subject *A*'s consistent results, it is probable that his attitude and his criteria of judgment remained fairly constant. But from what has recently been found in regard to the effect of attitude on limen,[1] it is useless to discuss individual differences in regard to sensitivity, unless one is sure that the attitude is constant or is cognizant of the nature of the change.

The ranking of the five subjects according to their M. S. for the three methods is:

Right hand	*E D* (*A B*) *C*
Simultaneous	*E C A D B*
Successive	*E C A D B*

There is perfect correlation between the two methods for the two hands. The change of rank appears in the change from one hand to two hands, the degree of correlation being only .18.

The exceedingly low, in fact very improbable M. S. of .01 mm. of subject *E*, makes one suspect that in his case fifty judgments for each pair were not sufficient. It seems too low even though his nervous jerky manner in holding the stimuli may have changed the simultaneous presentation into a quick and repeated alternation of stimuli.[2]

[1] See Fernberger, *op. cit.*, and George, *op. cit.*

[2] While on the subject of the number of judgments, it might be said that the decrease of all the coefficients of precision for the two-hand judgments is an indication that it would have been better to have made more judgments for these series.

Another point of interest is that the S_2 is further from the objective equality than the S_1, in practically all cases. This indicates the constant error or general tendency of judgment and the shifting here is in the direction of the larger judgments. If more subjects had been used, it is possible that there would have been those who preferred smaller judgments and those who showed no preference, that is, negative and indifferent types as well as positive types, such as Martin and Müller found with lifted weights.[1] There are certainly not sufficient cases to conclude that only the positive type exists in determining this limen, although this type seemed to predominate in most of the previous experiments. On the other hand, one of the explanations offered relative to the lifted weight experiment, namely, that the types depend upon the strength of the subject, is hardly applicable here, unless it happens that one develops a general set in the course of judgments depending primarily upon strength and that then this set influences all judgments of greater or less.

In Table II. are recorded in percentage all *a*, *b*, and *c* confidence according to right and wrong judgments. The figures are calculated from the data of all six subjects. When the $d = 0$, there is little difference in the per cent. of *a* judgments for the larger and smaller judgments. The figures are 21 per cent. compared to 17 per cent. But when $d = 1$ mm., many more correct answers have *a* confidence than incorrect, 20 per cent. and 25 per cent. correct to 8 per cent. and 9 per cent. incorrect. When $d = 2$ mm., this difference is still greater, 31 per cent. and 34 per cent. correct to 10 per cent. and 9 per cent. incorrect. The distribution of the *b* judgments is in the same direction, but the differences are not so marked.[2] It is evident that the mere judgments

[1] 'Zur Analyse d. Unterschiedsempfindlichkeit,' Leipzig, 1889. Fullerton and Cattell among others found that the judgments were more often right when the second weight was heavier. 'On the Perception of Small Differences,' University of Pennsylvania Publications, Philosophical Series, 2, 1892, p. 127. It seems impossible entirely to eliminate the time and space errors and obtain symmetrical curves. If other investigators had not found a similar lack of symmetry we should have thought we had perhaps too few judgments. Even so, fifty judgments seem the minimum and one hundred would be better.

[2] Pearse and Jastrow found a similar relation between confidence and correctness.

'greater' or 'less' do not give a complete picture of the effect of the difference. If we had used categories such as 'much greater' and 'much less,' probably the differences which here were expressed in a subjective scale would be expressed in the actual judgments. Suppose 'greater' was the correct judgment, then if conditions were the same as in this experiment, there would have been more 'much greater' judgments than 'much less' judgments, if these categories had been added.[1]

TABLE II

Com. Stim.	*a*	*b*	*c*	*a*	*b*	*c*	*a*	*b*	*c*
		<			=			>	
5	66	111	201	0	16	130	71	106	156
d = 0	17	30	53	0	10	90	21	32	47
		< (wrong)			=			> (correct)	
5.2	12	44	63	0	8	91	198	225	223
d = 2	10	37	53	0	8	91	31	35	34
		> (wrong)			=			< (correct)	
4.8	8	19	62	0	19	115	212	191	221
d = 2	9	21	70	0	14	86	34	31	35
		< (wrong)			=			> (correct)	
5.1	16	73	115	1	23	118	105	180	217
d = 1	8	36	56	0	17	83	25	25	50
		> (wrong)			=			< (correct)	
4.9	15	48	101	0	14	145	106	185	241
d = 1	9	30	61	0	9	91	20	35	45

a, *b*, *c* = degrees of confidence. The first horizontal column of figures for each comparative stimulus shows the number of judgments for each degree of confidence and for each class of judgments. The second horizontal column shows the per cent. of each of the three degrees of confidence for each class of judgments.

See Jastrow's A Critique of Psychophysical Methods, *Amer. J. of Psychol.*, 1887–1888, p. 305. Fullerton and Cattell also found that the degree of confidence varies as the percentage of right guesses. *Op. cit.*, p. 126. Harold Griffing, in his article on Sensations from Pressure and Impact, Psychol. Rev. Monog., 1895, p. 45, reports the same relation. As there were very few *a* judgments, this relation holds for the *b* judgments. G. M. Whipple, in An Analytic Study of the Memory Judgment and the Process of Judgment in the Discrimination of Clangs and Tones, *Amer. J. of Psychol.*, 1901, p. 446, also found much greater certainty for right answers.

[1] H. Ebbinghaus advised the use of the following categories: deutlich kleiner, ebenmerklich kleiner, gleich, ebenmerklich grösser, und deutlich grösser, 'Grundzüge der Psychologie,' Bd. 1, 1892, p. 74.

It is also interesting to note that the equality judgments were never given with confidence and the amount of *b* confidence is less than with the other judgments.

The subjects, ranked in order of confidence according to the number of *a* judgments, are: subject *E*, 311; subject *A*, 280; subject *D*, 129; subject *B*, 38; subject *C*, 37; subject *F*, 15 (see Table III.). It is seen that only subjects *E* and *A*

TABLE III

DISTRIBUTION OF THE JUDGMENTS OF EACH SUBJECT ACCORDING TO CONFIDENCE

Confidence	Subject *A*	Subject *B*	Subject *C*	Subject *D*	Subject *E*	Subject *F*
a..........	280	38	37	129	311	15
b..........	168	170	230	270	273	174
c..........	298	448	472	334	162	478

had a high degree of confidence. Subject *D* had a medium degree, and subjects *B*, *C*, and *F* very little. This assumes that they are using the same scale. Even though one subject's *a* means a higher degree of confidence than another's, however, it seems as if the number of *a*'s should give some indication of the subjective state. There is the usual lack of agreement between accuracy and confidence. Subject *A*'s results do correlate positively, but subject *B*'s correlate negatively. On the other hand the correlation between the M. S. for the right hand and degree of confidence is high, being .83. The correlation between the M. S. for the other two forms of presentation, simultaneous and successive, and the degree of confidence, however, is only .40. As one would suppose, we do find a certain correlation between lack of confidence and the number of doubtful judgments. Subjects *B* and *F* have the most doubtful judgments. They also are low in confidence. On the other hand, because a subject lacks confidence is no indication that he will have many doubtful judgments, as is shown in the case of subject *C*, who has only ten doubtful judgments and only thirty-seven *a* judgments out of seven hundred and fifty.

It will be recalled that the subjects were instructed that when they could not answer 'larger,' or 'smaller,' or 'equal,' they were to record the fact by naming it a *d* judgment and

then guessing either larger or smaller. There was found to be among all the judgments made in the half-year, including the preliminary trials of the main series, two hundred and twenty-nine such guesses when there was an objective difference. Of these, one hundred and sixty, or 70 per cent., were correct, and sixty-nine, or 30 per cent., incorrect. There were forty-nine guess judgments with objective equality, namely, when $d = 0$. Here, in contrast with the above distribution, there are almost an equal number of larger and smaller judgments, namely, twenty-six and twenty-three. These results conform to the laws of chance, although there are too few cases to be at all convincing, except in connection with the other results. Most of the guess judgments were divided between subjects *B* and *F*. Subject *B* had 72 per cent. correct and 28 per cent. incorrect; subject *F*, 71 per cent. correct and 29 per cent. incorrect. All the others showed the same tendency, but the numbers were too small to have any value except to strengthen the evidence of a predominance of correct answers by leaning in the same direction. It is clear, therefore, that although the subjective state of the subject was one of extreme doubt with no conscious attitude to swing him one way or the other, if he made a judgment without deliberation or hesitancy, he would have had many more correct answers than incorrect.

In thus making 'greater' or 'less' judgments, the subject probably assumes an attitude more closely resembling his set toward the other judgments. The 'doubtful set' is undoubtedly a change of attitude toward the experiment and should not be encouraged. It seems as if the instruction to guess 'larger' or 'smaller' gradually brings the subject into a constant attitude or to one approximating it. This seems to be borne out by the decrease in doubtful judgments. George says that [illegible] tended in the effort to maintain [illegible] attitude to make passive judgments [illegible] attitude when one gues[illegible] [illegible]ly like the pass[illegible] [illegible], deliberate [illegible] [illegible]ys further

that "the indication is that the way to maintain a constant attitude is to dispose oneself to make passive immediate judgments."[1] Our experience seems to support this. One's attention is diffused, as it were, over the experience, rather than concentrated now on this, now on that, criterion, and the response comes, it often seems, almost involuntarily. One is often surprised that the judgment is correct. There seems to be little doubt that the subjects of the psychophysical experiment should be trained in this attitude.[2]

In this connection, subject *B*'s two sets of results are of value. The ones in brackets, in which the doubtful judgments have been treated as equal judgments, give higher M. S.'s and lower precision than the results in which these doubtful judgments have been ruled out and judgment of *a*, *b*, or *c* certainty substituted. From the relation of the right to wrong judgments for this set, it is evident that if, instead of ruling out the 'guess' judgments and substituting others, the 'larger' and 'smaller' guess judgments had been used, the results would have been similar to those obtained from the former procedure and which have been used in the table unbracketed.

R. S. Woodworth recently reopened the question of the use of the equal and doubtful category.[3] He says on page 66 that "the main point is, of course, that the middle class of judgments, consisting of 'equal' and 'doubtful' judgments, is only a subjective definition, varying from individual to

[1] *Op. cit.*, p. 33.

[2] Several investigators have analyzed guess judgments and our results are in agreement with theirs. Pearce and Jastrow, On Small Differences of Sensation, *Memoirs of the National Academy of Sciences*, Vol. 3, 1884, found 60 per cent. correct guesses. Fullerton and Cattell found 60 per cent. and 65 per cent. correct judgments for two subjects. *Op. cit.*, p. 132. Whipple says, *op. cit.*, p. 83: "At the same time we found the subject had overlooked this element of his field of sensation, although his attention was directed with a certain strength toward it, so we marked his confidence as 0. This happened in cases where the judgments were so much effected by the difference of pressure as to be correct three times out of five." Griffing, *op. cit.*, p. 46, found the average of all correct guesses was 59 per cent. "In the case of some observers whose confidence was small, the percentage ran as high as 65 per cent. and 70 per cent."

[3] Professor Cattell's Psychophysical Contributions, *Arch. of Psychol.*, 1914, 30, 60–74.

individual. One individual insisting on a rather high degree of clearness or confidence for his 'greater' or 'less' judgments will throw a large portion of the cases into the middle class, while another individual will follow slighter indications and throw similar cases into the plus or minus column. The individuals differ in their standards of confidence, but not necessarily in their power to perceive a difference. Now since it has been proved that the division of the middle class between the other two classes results in a good balance of correctness, the individual who takes the more chances or has the lower standard of clearness will get the better record." Woodworth therefore continues to advocate forcing the subject into the false position of inhibiting equal and doubtful judgments in order to make results of all subjects comparable. This end can probably be attained better by the method outlined in this paper. In regard to the equal judgments, as was said before, none of the subjects seem to have had difficulty in getting the positive perception of equality (except the one subject with the preëstablished set, and even he made many such equal judgments). The doubtful judgments are allowed, but not included in the results.[1] The results showed no indication of avoiding a positive decision or of lapsing into a state of mild 'aboulia,' as Warner Brown intimated would happen under such conditions.[2] If the doubtful judgments were allowed to remain with the equal judgments in the results, Woodworth would be justified in saying that "the boundaries of the middle class are fixed by each subject without objective control and almost invariably differ from subject to subject, etc.,"[3] for we saw how many categories the *d* judgments included. By counting only the very definite category of 'equality' in the sense of a decided impression of equality the boundary is very firmly fixed. By allowing the subjects who so desire to make doubtful

[1] G. W. Whipple repeated the doubtful tests until the observer made satisfactory judgments, but the number and distribution of these cases were recorded. *Op. cit.*, p. 417.

[2] The Judgment of Differences, *Univ. of California Publications in Psychology*, 1910, 1, 32–33.

[3] *Op. cit.*, p. 65.

judgments we gain two points. We keep the boundary sharply defined in his consciousness and by noting such judgments are able to make and allow for a change of attitude. These doubtful judgments tend to grow less with practice and would probably disappear in the latter half of a series of one hundred judgments. In other words, the method of impressing upon the subject the difference between judgments based on the actual perception of equality and those called equal because there was no judgment one way or the other probably had the beneficial effect of preventing too many equality judgments, and the knowledge that guess judgments would not stand as such kept the subject keyed to the proper amount of attention and probably prevented him from shifting his attitude.

Summary

1. The measurement of sensitivity for finger span with a standard of 5 cm. for the two most reliable subjects was .45 mm., or about 1 per cent., when one hand was used. It was in most cases higher when the stimulus was presented to the two hands, whether successively or simultaneously, but for one of the two most reliable subjects it changed very little. His M. S. remained relatively low for all three forms of presentation.

2. There was no decided agreement between subjective confidence and reliability. The best subject did have a high degree of confidence, but the second best had a low degree.

3. Decidedly more right judgments had a higher degree of confidence than wrong judgments.

4. When the subjects were forced to make judgments which had the quality of guesses, there were many more right than wrong answers.

The Judgement of Emotions from Facial Expressions

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JUDGMENT OF EMOTIONS FROM FACIAL EXPRESSIONS

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Y interest in the question of the judgment of emotions from facial expressions, and of the validity of first impressions led me to arrange an experiment in the Harvard Psychological laboratory whereby the ty to read emotional expressions could be tested. Among r data, certain facts concerning the nature of emotions, r relation to each other, and the various forms and degrees lifficulty of interpretation were obtained, and have seemed of ficient general interest to be described here.

The ideal method would have been to judge actual facial pressions under emotional strain, but that was obviously im- ble in the laboratory. A cinematograph, would have been ost as good and more practical, but not having such an in- ment, I had to resort to pictures. From Rudolph's "Der ruck des Menschen," which contains 680 pictures, I cted 105 of the best. These pictures were sketched from tographs of a skillful actor who posed for that purpose.

The pictures were shown to two subjects at a time, there ng in all four men and two women. They were asked to write their interpretations of the emotions portrayed. About ty judgments were made at one time. After the entire num- had been gone through, the pictures were shown a second e, but not in the same order. The subjects were again asked write down their judgments. This was to discover the degree onstancy. They were then told the judgments of all of the jects, and also the book titles of the pictures. This was to over if they would recognize their own judgments, and also they would select the book title. They were then told the k title, and asked if they preferred that title to the judgment had made. In this way the accuracy of portrayal by the st could be ascertained. It may be stated here that the jects were fairly consistent, that they did not very frequently

recognize their own judgments, and that there were ty
differences in their ability to judge such expressions. I
only taken those pictures for further discussion whose book

1 2

3 4

was accepted by at least three of the subjects. It is fai
assume that this 50 per cent acceptance indicates a succe
portrayal by the artist. Of the other pictures, upon whose

ly two or less agreed it would be difficult to say how much the lgments were influenced by the deficiencies of the artist.

Although I have spoken of the expressions as emotions, the es to be analyzed will include what generally are termed noods" and "sentiments." It is often very difficult to make listinction between these latter and emotions, as the one passes idily over into the other. Also we are concerned only with eir outward expression and not with their significance and irce.

In most instances complex emotions, usually with one minant emotion, have been portrayed, and it will be es-cially interesting to note how well this complex expression has en analysed. Let us begin with the "amazement" group. a picture entitled "amazement, doubtful," all the subjects ted surprise, and several interpreted the "doubtful" by keptical." If the amazement is very strong, we have the pression of fear. Five of the subjects called strong amaze-nt either fearful surprise, sudden terror, terrified surprise, rror, or frightened surprise. McDougall writes—"Surprise merely a condition of general excitement which supervenes on any totally unexpected and violent mental impression," .[1] "Startled and unpleasant amazement" brought out horror d terror very strongly, while amazement which was mildly pleasant was seen as doubting, anxious, puzzled and perplexing prise. This puzzled expression is frequently seen in the mild azement as contrasted with the fear and terror of the strong otions. In "amazement with hate and scorn," the amazement s for most lost in the stronger emotion. Of the twelve lgments only four times was amazement or surprise seen. The e and scorn were seen as strong contempt, angry revenge, ong scorn and anger, mean, vengeful surprise, and resentful, agonistic surprise. The scorn also aroused the idea of istfulness and defiance.

In order to give the reader an idea of the accuracy of the lgments, four of the pictures used in the experiment have been roduced on page 173. The first of the pictures is supposed represent "speechless amazement."[2]

[1]Social Psychology. Pg. 157.

[2]The 12 judgments for this picture, that is, the first and second judgments of each of the six ects, are as follows:— (1) Frank surprise, (2) Unsophisticated wonder with fear, mild; tupefied alarm, (2) Surprise; (1) Mingling of surprise and slight displeasure, (2) Dumfounded, frightened; (1) Skeptical surprise, (2) Surprise, strong apprehension; (1) Stiffened amaze-t, (2) Strong amazement with slight fear; (1) Stupefied alarm, (2) Startled surprise.

The anger group was represented by very strong, peevis anger, and anger, laughing against the will. The feeling of pai distress, tormented fear, and grief were seen in both picture more frequently than the more aggressive attitude of ange This apprehension of what might be called "centripetal" feelin such as fear, pain, distress, instead of the more violent aggressiv emotions such as anger and hate occurred frequently throughou the experiment. Intimation of peevishness is seen in the judg ments of fretful pain, complaining and distress, about to cr The attitude of "laughing against the will" was noticed by tw of the subjects, and interpreted as transition from mirth int pain and distress, and laughing and crying.

A "mild, impotent hate" was judged several times as a feelin of fear, pain, sorrow, anxiety, and discontent. Hate was onl mentioned once, but the book-designation was accepted by thre subjects. Unqualified hate was frequently judged as anger, an several times as scorn and contempt, but never as hate. O the other hand, "vindictive hate" was seen by five subject Anger was here also shown to be closely related to hate, and th vindictiveness is seen in judgments such as snarling revenge an extreme malice.

The malicious mood accompanied by vindictive laughte was frequently interpreted as hate. The malice appeared in su adjectives as nasty, gloating, hateful, vindictive joy, triumphan hate, and spiteful rage. This mood seemed very well represent in the facial expressions.

In the disdain, contempt, and scorn group, disdain w represented together with very mild pride and superiority, greater pride and superiority. In the first case, the superiorit and pride were noticed as smug complacency, arrogance, haught assurance, egotistical calm. The disdain was not seen until was pointed out. In the other case, however, when the pri was more strongly represented, the disdain was seen at once, an interpreted by some as disdain, but more frequently as contem sneering, and scornful arrogance. This seems to indicate t importance of pride in the attitude of disdain. When contem is portrayed accompanied by a mildly ironical and su smile, the superior smile gives rise to the judgments of satisfaction and scornful arrogance, and pleasure in trium but the contempt was not seen until the title was shown, exce perhaps, in the case of scornful arrogance. When the conten was portrayed with diabolical laughter, the diabolical laugh

was frequently correctly interpreted, but again the contempt was not seen until the attention was directed to it. It seems evident that in both cases the laughter drew the attention from the more subtle attitude of contempt. An interesting picture represented the attitude which can best be described by the phrase "I am sorry, but I cannot help you." This was judged as contempt, disdain and arrogance. This attitude is seen in the uncharitable, where it is so frequently a mixture of scorn and a suspicion of self-satisfaction, which latter was also noticed in this picture. In the picture of "scorn with wicked laughter," anger is prominent in the judgments as well as hate. This seems in accordance with what McDougall says—"When an object excites our disgust and at the same time our anger, the emotion aroused is scorn."[3] In scorn with cynical, mild contempt, the scorn was seen by several, but the cynical attitude was observed only when pointed out, and then four of the subjects agreed with the title.

A number of pictures were shown representing misgiving ith various combinations. When the attitude was mild, it was interpreted frequently as surprise, when stronger, together with surprise, it seemed liked distrust, perplexity, and also fear and anxiety. One picture portrayed misgiving together with meditation upon evil. It was the latter attitude which was noticed, and described as plotting, disgruntled discontent, brooding, sullen, and vindictive. Perplexity was the only description here which referred at all to misgiving. The same can be said of misgiving with a meditative, fixed stare. Here, also, perplexity is seen, as well as contemplative thoughtfulness, fierce contemplation, perplexed consideration, etc. In the picture entitled, "misgiving wavering between jest and earnest," the conflict, as in the previous instance of conflict, was seen by several of the subjects.

In the picture of fear, which was entitled "fear with a wicked, evil conscience," the fear was seen and also the surprise which accompanies fear, and, just as fear was seen in anger, so anger was seen in fear. The second picture on page 173 depicts "fear and horror."[4] The picture of "very strong terror" also gave rise to veral judgments of anger. The terror and fear, at times

[3] Opus cit. pg. 135.

[4] The judgments were—(1) Excessive pain and suffering of self or others,(2) despondent over another's danger or disaster; (1) Pain, (2) Horror, surprise; (1) Acute pain with fear, horrified anguish, contempt; (1) Anger, (2) Apprehension, terror; (1) Terrified fear, (2) In-e fear; (1) Rage, (2) Terror from pain. Here also anger and rage are seen with fear.

accompanied by amazement, were readily seen. In the picture portraying "strong anxiety and horror," the stronger reaction of horror diverted the attention from the attitude of anxiety so that no one saw the latter until the title was given him. Again, as with fear and terror, the accompanying surprise was also noticed.

Aversion was represented by "aversion with strongly suspicious fear" and "by very ordinary aversion accompanied by nausea." In the former picture, hate, contempt, anger, and disgust predominated. The fear was also noticed by several, but there was no impression of suspicion conveyed. In the latter, aversion was seen as dislike, scorn, contempt and disgust. Nausea was only noticed by two subjects, but all six subjects accepted the title when told. The ideas of fear and of anger were also to some extent aroused. That these two emotions should be seen in a very strong expression of aversion is to be expected. They are entirely lacking in the milder form of aversion which is shown in the third picture on page 173. Here, contempt and disgust predominate.[5]

The distrust group, like the preceding aversion group, conveyed the attitude intended. "Simple distrust and doubt" were seen as suspicion, bewilderment, questioning contemplation, and puzzled reflection. The "distrust with suspicion and fear" aroused in most cases suspicion, apprehension and anxiety. Only one judgment mentioned distrust in this setting. The third picture represented distrust with strong biting of the under lip. In this case, distrust was only mentioned twice. The lines of the lip combined with the expression of distrust, gave rise to judgments of determination with evil intent, such as plotting, angry determination, malice, and plotting determination, etc. The fourth picture on page 173 represents "very keen distrust."[6] One picture represented sinister determination, animosity. The determination was seen by several subjects. The animosity gave rise to judgments of resistance, indignation, dislike, fierce and bitter determination. Five of the six subjects accepted the

5The 12 judgments are as follows:—(1) Disgusted contempt, (2) Scornful contempt accompanied by an inner feeling of slight puzzlement; (1) Contempt, (2) Dislike and disdain;(1) Medium strong contempt,(2) Contemptuous scorn; (1) Disgust, (2) Disgust; (1) Sneering contempt, (2) Mild dislike and disgust; (1) Sneering contempt; (2) Contemptuous aversion.

6The 12 judgments are as follows:—(1) Analytical, reflective with anger, (2) Inquiring speculation with critical constructiveness: (1) Concentration, determination, (2) Contemplation, planning, dislike; (1) Firm and mildly angry opposition, (2) Pugnacious contemplation; (1) Thoughtful doubt, uncertainty, (2) Doubt, distrust; (1) Unintelligent doubt, (2) Meditation, doubt and suspicion; (1) Scornful distrust; (2) Deep thought with suspicion. The book title itself in this instance was accepted only twice, due undoubtedly to the fact that the subjects' titles were more fully explanatory of the intended mood. The anger seen by [illegible] of the subjects was seen by these same subjects in the first picture mentioned in this group.

k title. There seems to be evidence that determination and ›rms of animosity are readily seen.

Laughter is probably depicted and judged more easily than ny other emotion. As it is a very strong emotion, the more btle states are often hidden. That is the case in the picture of suspicious laughter," for the suspicion is not seen as such, although iere is intimation of it in one of the judgments. Three of the ibjects, however, selected the title with suspicion when presnted with the other judgments.

In the picture of "bodily pain and screaming," the pain was en by almost all the subjects, and the screaming was judged as rying by two. It was also seen as anger and revenge by several.

When "entreaty with strong, cordial smile" was given, the ughter emotion predominated, and the entreaty was seen only y one subject, and that as smiling and begging. The entreaty as, however, noticed by three of the subjects as soon as attenion was called to it.

The picture of "sulky, defiant ill humor" produced uniform eactions. Although the mood, ill humor, was not mentioned the judgments, all six subjects chose the book title from among ie other titles. On the other hand, the more subtle mood of pessimism was accepted in one instance by only one subject, and i another by two, and referred to by no subject in his original udgment.

The attitude of devotion and reverence was judged as peaceıl meditation, religious contemplation, uplifting thought, etc, ut when the artist attempted to portray religious inspiration, e signally failed. Three pictures, supposedly representing this tate, were interpreted almost without exception as amazement, frequently accompanied by fear. It may be that this failure is caused by racial difference, the Teuton being prone to express uch moods in exaggerated form. McDougall says "Emotions hat play a principal part in religious life are admiration, awe ınd reverence."† The awe expressed as amazement and fear seems to be the only element of this religious attitude which the artist caught.

At the end of the series of experiments, the subjects were asked to describe the method by which they interpreted the experiment. Their interpretations offered sufficiently interesting points to be partially reproduced here. That which strikes one most forcibly is the close agreement of the various reports. The empathic response or sympathetic imitation of the facial

expression in the picture is the one almost invariably adoptec There is frequent reference to kinaesthetic sensations of imitatio in the observer's own face. Rarely, however, is this facia imitation sufficient in itself to establish a satisfactory judgment Five of the six subjects referred to the fact that they had t imagine an entire situation which would involve response of the entire organism. The individual of the picture wa imagined in a certain definite situation such as being faced b some one against whom he could show his anger. At times th observer consciously imaged himself in the scene. It is probabl that at other times he unconsciously identified himself wit one of the group. The emotion, at least in its incipient stag was aroused in him by this participation.

In some instances an association was formed between th picture and some well known painting expressing definite emo tions or else situations which had actually occurred in the ex perience of the subject were recalled.

Only one subject denied any active participation. That th introspection of this subject did not discover such an attitud is not positive proof of its non-existence, partly for the reaso that she was not very well trained in such reports. An ex planation of her exceptional attitude, however, is suggested b her own description. She had all through her life made a stud of faces, and had thereby made close associations between certai emotions, personal characteristics, etc., and the various lines o the face. It is possible, therefore, that through this habit th emotions were more directly observed than in the case of th other subjects. It is needless to state that the other subject also must have made some direct response, either throug direct empathic imitation of the features, or through associatio with some known situation; otherwise, they could have ha no clue by which to start their imagery. That is, there must hav been some impression before imaging a situation in order to giv it the proper setting. The situation would then offer them opportunity to experience more subtle shades and combinations, and to correct their first impressions.

Mention was also made by several subjects of the fact that they got their clue from certain features to the exclusion of others. It is fair to assume that this frequently occurred, especially whe certain features were more strongly affected than others, an that this caused a wrong, or at least only partial, judgment. I it not just this habit of being attracted by the more pronounce

lines of the face that so often causes one, in actual life, to misread character? Frequently we are deceived into judging by the stronger and more transient expressions, and neglecting the subtler and, perhaps, more important lines

The following are extracts from the introspections. One subject writes: "Frequently the expression was visualized as appearing upon the face of some person of my acquaintance, and the effort was made to estimate the circumstance that would call forth that expression. Where the picture shown represented an expression of common occurrence or of not too detailed a character, this general idea was enough to indicate the estimate of the picture, the judgment being rendered very promptly. A few times I was conscious of a definite attempt to reproduce the expression in my own countenance in order that the resultant lines and sensations might aid in getting the meaning. Frequently the expression was analysed, the effort being made to determine the meaning of the eyes by themselves, then the mouth, etc. The result would sometimes be a unit, often a positive, complex description. The judgment was frequently made by trying to imagine what was before the eyes of the man in the picture, that is, trying to imagine the circumstances which would be apt to produce such a facial reaction."

A second subject writes: "For the most part the particular face was inserted in a visually imagined episode; and the judgment of the individual's emotion made according to what would necessarily be involved in the given situation. Sometimes there was an addition of auditory images of what the individual under the circumstances might say. A second method employed now and then involved imagining the individual and myself alone in a situation. In this case, also, the auditory images perhaps more often played a role. In some further cases, the two methods were combined; the method being to imagine a situation involving the individual with several others and myself either with or without the auditory imagery. Rather often all of these methods included specific memory images of actual experiences in which individuals gave vent to certain expressions and emotions. In almost every case, also, there was a distinct kinaesthetic imagery in myself, especially, the muscles of the face: a slight tendency to mimic the expression in the picture. Almost always this was involuntary. Actual expressions almost resulted, often before I was conscious of the strength of this imitative impulse. This was most common

in the cases when I imaged myself as a member of a group in a situation, from which I reasoned by analogy the emotion of the individual in the pictures . . . "

A third subject records: "In judging the facial expression of the pictures I find that in most cases I look first at the mouth, and jaw. When I have secured an impression of any kind, I have a tendency to set my own jaws and lips in the same attitude and with something of the same degree of muscular tension. This done, I imagine a situation in which I might be disposed to use such a muscular set. Almost always I employed words to suit the case, and imagined other people to whom I was reacting and a situation in which we were placed. I think it is fair to say that there was practically always a social aspect to my perception of the picture. In many cases the kinaesthesis extended into my arms and thorax, if not throughout the whole body. It is safe to assume so, at least, for I am strongly kinaesthetic at all times.

After the first inspection of the lower part of the face, I took in the nose, cheeks, and then the eyes. I can see now that out of this procedure came the basis on which I sometimes changed later. I was set at first by the lower half of the face; the set was strengthened by use of words and an ensuing kinaesthesis; then when I took in the eyes and forehead, I was not so ready to give them their due weight as I should have been. I was influenced toward this action by the—to me—very remarkable lips and jaw of the man whose face was pictured. They struck me from the first and took the center of consciousness, almost always, immediately the picture was disclosed. When it came to the second presentation of the picture, I was frequently made to see, by the judgments of other subjects, that I had not given all parts of the face due consideration. In deciding whether to change or keep my own judgment I was therefore called on to determine whether I had given due value to all the factors in the presented expression. Sometimes I found elements that had been overlooked, and then I changed. If I did not find any, I tended to stand by my original statement."

A fourth subject writes "Sometimes I tried to think of myself with that particular facial expression, but usually I thought of that expression on another face, and tried to think when and where I had seen a similar expression before. There were certain expressions of the eyes and mouth which I also remembered as having been taught meant certain definite things.

A fifth subject says "In judging the facial expression, I almost invariably imagined, and always in visual terms, some situation in which the character might have assumed such an expression. Then, very frequently, I thought of my face screwed up or composed in that way, and how I would have to feel to assume such an expression. This imagery was so strong that I frequently noticed my facial muscles twitching. At times, but rarely, I consciously and deliberately assumed the expression of the face in the picture. This was usually in the case of expressions that were difficult to judge. At other times it was like seeing a tableau with one of the actors looking like the picture, and then trying to imagine what the others were doing to make him look like that. I noticed many associations with the story-books I had read and incidents of my own life. Especially was this true at the beginning of the experiments. Judgments at the latter part were less intensely personal. Visualization was not so vivid and at times almost absent."

The sixth subject, who was the one mentioned above as not having kinaesthetic imitation, remarks "It is easier for me to tell the methods I did not use in judging the facial expressions than the one I did. I could not get any clue as to the emotions expressed by imitating the facial expressions or trying to imagine how I should feel if I looked like that. Neither could I get any idea by thinking of other people. All my life I have studied peoples' faces in cars, theatres, restaurants, to see what characteristics they showed. I never used comparisons, but always looked first for the general idea, then analysed each feature and line to see what each contributed to the general impression. I had never seen, in actual life, such strong and violent emotions as the man expressed, so I merely considered him as an individual, and used the same methods as I employed on living people. Some pictures reminded me of paintings and portraits I have seen, carefully labelled by the artist as to the emotion, and this was a little help in knowing how others regarded the emotions they were expressing."

The similarity in description of these subjects is so marked that it might be suspected that their introspection had been suggested by the experimenter. The question put to them in written form was simply "Describe as fully as possible the manner in which you judged the facial expressions." Without consulting anyone, they wrote out their description, and handed it to me, and it has never been shown to anyone.

In conclusion, the following brief summary may be added. It did not need this experiment to prove the well known fact that emotions and attitudes can be judged from pictures. That the judgments would be so uniformly good and consistent, however, even when made by individuals who, with a few exceptions, had never assumed any particular aptitude in this direction, is of some interest in the study of emotions. Even many subtle combinations of emotions were observed, and conflicts such as half crying and laughing, or between jest and earnest, were noticed.

Laughter was, almost without exception, observed. Anger, fear, and hatred were also most expressive in all their forms. The intimate relation of anger and fear was brought out. Fear was seen when anger predominated and anger when fear was uppermost. Fear was also seen in amazement.

Hatred was frequently interpreted by the more active, attitude of anger and contempt resulting from it. Suspicion was often judged as surprise. The contempt and scorn group was unmistakable, the lines of the mouth and also part of the nostril rarely escaping attention, or missing their effect.

When a combination was presented, the stronger emotion frequently inhibited the perception of the more subtle and, sometimes, more important one.

Certain moods such as sullenness and peevishness were correctly interpreted. It could hardly be expected, however, that temperamental attitudes such as covetousness and pessimism would be seen. It is the effect of these impressions that is observed; the temperaments themselves are judged by inference, and the subjects were either incapable or, at least, not instructed by the general nature of this experiment to make such deductions.

The reports of the subjects showed that they obtained their results frequently by kinaesthetic imitation, and by association with known experiences, and by the imagining of situations which would give rise to such emotions. Modern psychology teaches us that cognition is an active attitude toward an object, and although we are not conscious of it in judging our fellow beings, our organism is undoubtedly adjusted according to the clue afforded by the expression. In the field of art, empathic imitation is practically universal. Here, not only do we have the facial expression, but also most frequently the situation. The results of the introspection give some indication of the

nportance of the surroundings in a picture for the conveyance f the true message in the face.

In a subsequent paper,[7] results of further experimentation ill be recorded. It will be shown how wide is the range of »ility in interpreting emotion thus expressed. It will also be own how very suggestible some individuals are in this regard.[8]

7To appear in the Psychological Review.

8Miss Feleky, in an experiment similar to the one described, obtained some results in accord ith ours. She found that the aversion, contempt, sneer group was easily observed, hate brought it disgust, and rage gave terror. She found that fear was part of suspicion. Laughter was so easily observed by her subjects. In our experiment, determination was frequently correctly terpreted. Miss Feleky's results are not in accord on this point, probably due to the pictures used. (The Expression of Emotions, A. M. Feleky, Psychol. Review, 1914, pp. 33-41.)

JUDGMENTS OF FACIAL EXPRESSION AND SUGGESTION

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JUDGMENTS OF FACIAL EXPRESSION AND SUGGESTION

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In an experiment upon the judgment of emotions from facial expressions,[1] in which the subjects attempted to name the emotion depicted in a number of photographs, it was noticed that it was at times possible under suggestion to read various emotions in the face. An experiment was therefore arranged to determine to what degree one is open to suggestion when reading character. It was also the aim of the experiment to bring out, if possible, individual differences in the power of interpretation of facial expression and in suggestibility. The same pictures were used as in the former experiment. Four of the pictures are reproduced in Plate I. These were modified photographs of a talented actor, which appeared in the book by Rudolf entitled 'Der Ausdruck des Menschen.' One hundred and five of the pictures were selected as being the best for the experiment. They covered a wide range of emotions and moods. A few of bodily pain and of the sensations of smelling and tasting, and so forth, were included. Miss Grace Speir conducted the experiment, and tabulated the results. There were five subjects, who were all either members of the advanced experimental course or graduate research students.

The experiment extended throughout the second semester of 1916–1917. Each subject came for one hour a week, and as many pictures as possible were presented in that hour. The subject was shown the picture, and asked to write down his judgment of the expression. After he had done this, he was told either the artist's title of the picture, or an incorrect title, such as 'inspiration' for a picture of 'distrust,' and asked whether he agreed with this title. Some of the incorrect

[1] A report will appear in the *J. of Abnorm. Psychol.*

PLATE I

3

L. 3 F

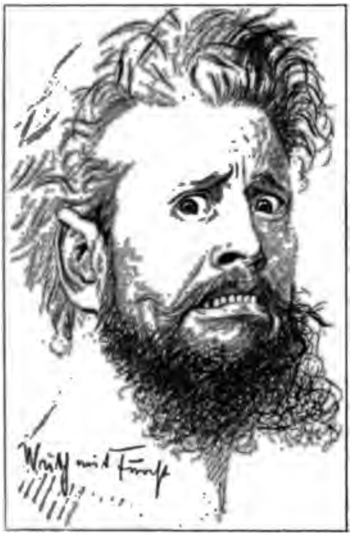
Bedenklichkeit. Kritisch

401

XXV. 1 A

Hohn. Leicht

510



XXVI. 67 A

Wut. Mit Furcht
(leichter Ausdruck)

585

XXIX. 1 F

Lachen.
Bedeutungsvolles Lächeln

FIG. 1. Four of the pictures used. They represent: 3, doubtful, critical; 401, mild scorn; 510, rage with fear; 585, significant smile.

titles were as opposed as possible to the correct title; others were rather similar. The series of one hundred and five pictures was gone through twice. At one presentation the subject was told the correct title, at another presentation the incorrect. On some pictures the correct title was given at the first presentation,—on others at the second presentation, so that the subject never knew, even if he did suspect the purpose of the experiment, whether a right or wrong title was being suggested.

The results are contained in Table I. The various groups of emotions used are shown in the first column. In the second column is the number of tests for each group of emotions. As there are five subjects, these numbers must be divided by five to give the number of pictures in a group; for instance, the scorn-contempt group has 80 judgments and 16 pictures.

The third column shows the number of times the subjects approximated the actual title of the book. An approximated

TABLE I

1	2	3		4		5		6		7		8		9		10	
Expression	Number of Judgments	Book Title Approximated		Of Column 3 Book Title Later Approved		Of Column 4 Wrong Title Later Suggested and Approved		Book Title Not Approximated		Of Column 6 Book Title Later Approved		Of Column 7 Wrong Title Later Suggested and Approved		Total Book Title Approved		Total Wrong Title Suggested and Approved	
		%	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%	No.
Scorn-Contempt.	80	36	29	96	28	21	6	64	51	80	41	54	22	86	69	41	28
Misgivings......	65	32	21	100	21	24	5	68	44	63	28	21	6	75	49	23	11
Aversion-Hate...	55	38	21	100	21	42	9	62	34	70	24	62	15	82	45	53	24
Amazement.....	55	58	32	90	29	21	6	42	23	60	14	71	10	78	43	37	16
Laughter........	50	64	32	100	32	31	10	36	18	55	10	70	7	84	42	40	7
Distrust........	45	31	14	92	13	61	8	69	31	54	17	71	12	67	30	67	20
Anger-Rage.....	30	30	9	88	8	0	0	70	21	57	12	25	3	67	20	15	3
Anxiety—Fear-Terror......	25	36	9	88	8	37	3	64	16	68	11	45	5	76	19	42	8
Inspiration......	25	8	2	100	2	100	2	92	23	47	11	64	7	52	13	69	9
Covetousness....	25	4	1	100	1	0	0	96	24	45	11	36	4	48	12	33	4
Wicked-Ill-tempered....	20	15	3	100	3	33	1	85		64	11	45	5	70	14	43	6
Begging-Entreaty	10	10	1	100	1	100	1	90	9	77	7	57	4	80	8	63	5
Bodily Pain.....	10	50	5	100	5	0	0	50	5	40	2	0	0	70	7	0	0
Smelling-Tasting, etc.........	30	37	11	100	11	9	1	63	19	78	15	73	11	87	26	46	12
Average-Total.	525	32	190	97	183	34	52	68	335	61	214	49	111	73	397	41	153

title was taken as a correct judgment because it could not be expected that the subject would frequently use the exact words of the author in describing the picture, so that 'approximated' means 'equivalent.' Throughout this table, the data are presented both in actual amounts and percentages. In the 80 judgments of the scorn and contempt group, for example, 29, or 36%, were correctly approximated. It will be seen that the laughter group was the most readily interpreted, and that amazement comes next. Scorn and contempt, misgiving, aversion and hate, disgust, anger and rage, anxiety, fear, and terror, all of which are more or less related, are interpreted with about the same accuracy. Inspiration, covetousness, wicked and ill-tempered, begging and entreating were poorly interpreted. It was not expected that the sensation groups would give so low a percentage of correct judgments. In this group were also included pictures of sneezing and yawning, and even these were at times incorrectly named. Of the entire 525 judgments a little over a third were correct.

In the fourth column is the number of book titles approved of those which had been already approximated, that is, if we take the scorn-contempt group, the artist's title for 28, or 96%, of the 29 pictures whose titles had been approximated was approved when subsequently shown by the experimenter. These figures give us a check upon the cleverness of the artist in portraying the desired emotions, and it is the data in the fourth column that are used as a basis in determining the suggestibility. A comparison of the totals in columns three and four shows that of the total of 190 such approximations only seven of the artist's titles were not approved when subsequently shown.

The figures in the fifth column show the degree of suggestibility for the various emotions. In the first group, 28 titles were approved. When the wrong titles came to be suggested for these same 28 pictures (after an interval of a month, on an average) six of these wrong titles were accepted. Of the 183 pictures whose titles had been already approved the suggested wrong title was accepted 34% of the time. Excluding the inspiration and begging-entreating groups,

which had only one and two judgments respectively as a basis for calculating the suggestibility, the distrust group offered the greatest opportunity for the effect of suggestibility, and the aversion-hate group was next. With anger and rage, suggestion had no effect.

The sixth column shows the number of times the subjects were unsuccessful at approximating the picture. These numbers are complements of the figures in column three.

Column seven gives the number of artist's titles which were approved of those pictures which the subjects themselves had previously not approximated. For instance, in the first group, of the 51 pictures which had not been approximated, the subjects approved 41 of the book titles. As was to be expected, fewer of the artist's titles were approved of these non-approximated pictures than of the approximated ones; in all only 61%, as against 97% of the approximated titles. This drop is indicative of the artist's failure to reproduce the expression he desired. The pictures that were not approximated, even though the artist's titles were approved, gave more room for suggestion than the approximated pictures. 49% of the suggested wrong titles of the 214 approved, non-approximated titles were accepted as against 34% of the 183 approved and approximated. This means that if the subject had not himself judged the title correctly, even though he agreed with the title that the artist gave, he was more open to subsequent suggestion than when he had judged the title correctly in the first place. Distrust is again high in suggestibility. The suggestibility in amazement is as high as in distrust, and in laughter almost as high.

In the next to the last column is the sum of all the book titles which are approved whether they had previously been approximated or not, and is obtained by adding columns seven and four.

In the last column is the total amount of suggestibility with the pictures from which the figures of the previous column were obtained, that is, the total amount of suggestibility whether the titles had previously been approximated or not. Of the total number of 397 titles approved, 153 or 41% offered opportunity for suggestion.

Table II shows individual differences both in the ability to judge emotions and in suggestibility. In the first horizontal line are the five subjects A, B, C, D, and E.

TABLE II

Subject	A		B		C		D		E	
	%	No.	%	No.	%	No.	%	No.	%	No.
Book title approximated	55	58	30	31	39	41	29	31	17	18
Of last, book title later approved	97	56	90	28	88	36	100	31	94	17
Of last, wrong title later suggested and approved	29	16	32	9	31	11	16	5	47	8
Book title not approximated	45	47	70	74	61	64	70	74	83	87
Of last, book title later approved	68	32	58	43	53	36	47	35	92	80
Of last, wrong title later suggested and approved	34	11	46	20	44	16	31	11	69	55
Total: book title approved	84	88	68	71	69	72	63	66	92	97
Total: wrong title suggested and approved	31	27	41	29	37	27	24	16	65	63

In the second horizontal line are the number and percentage of the book titles approximated by each subject. Subject A is the best and subject E the worst in correctly interpreting facial expressions. In order of merit they rank A, C, B, D, E.

The third horizontal line shows the number of book titles which had been previously approximated, and which were approved by the various subjects. For example, A approximated 58, or 55% of the total number. This column offers no new facts, but is used as a basis for calculating the amount of suggestibility which is shown in the next column.

Here it will be seen that subject E is the most suggestible having accepted 47% of the titles suggested with pictures whose correct titles he had approved; that is, when he was given incorrect titles in connection with the 17 pictures whose correct titles he had previously approved, he accepted eight of them. Subject D was the least suggestible. The ranking of the subjects in suggestibility is E, B, C, A and D.

In the fifth horizontal line is the number of book titles which the subjects did not approximate. This is the complement of the results in the second horizontal line. Therefore the ranking by subjects is the reverse of that of the second horizontal line.

In the sixth horizontal line is the number of book titles which were approved of those which had previously not been approximated. It is seen that subject E is the least discriminating, accepting almost every title shown him, that is, 80 or 90% of the titles suggested for the 87 pictures, previously not approximated. In this group of pictures whose titles were not approximated there are, of course, a great many pictures which very poorly portray the emotion intended. Undoubtedly, when the book titles were subsequently given, some of these which are really inappropriate were accepted through suggestion. If we rank the subjects according to the number of those book titles which they accepted, we shall see that the order closely correlates with that of the ranking according to suggestibility; the most highly suggestible accepting the most titles, and the least suggestible accepting the fewest. Inasmuch as subject E is also the most suggestible to wrong titles, it may be said that he accepts almost anything given him whether right or wrong.

The seventh horizontal line shows the amount of suggestibility to these non-approximated titles according to subjects It will be remembered from the first table where the results of the five subjects were averaged, that there was more suggestibility in the cases where the book title was not approximated. The figures of this line show that this is so in the case of each individual subject. The ranking according to suggestibility is the same as that previously shown with the approximated titles, being subjects E, B, C, A, D.

The last two horizontal lines present the average of the results of the approximated and non-approximated groups and offer no new facts.

It may be said that by this experiment decided individual differences have been shown among the five subjects in regard to suggestibility and ability to read facial expression. It is evident that subject E has very little ability in reading faces, and is highly suggestible, accepting 65% of the wrong titles shown him, as well as 92% of the book titles, some of which were decidedly poor according to the other subjects. It was discovered from other unrelated experiments and from the

subject's own report that he had little if any visual imagery, and that he felt entirely at sea when asked to make a judgment of emotions visually expressed. It does not follow, however, because one is unable to make correct judgments that one is highly suggestible, and will accept any title offered. Subject D, for instance, ranks next to E in inability to interpret correctly the emotions, but he accepts the fewest wrong titles. He also rejects the greatest number of titles in the non-approximated group. There is a suspicion that he is of the negatively suggestible type. The fact that he accepted all the book titles in the approximated group when they were read to him, subsequent to his judgment, does not contradict this, as he was here accepting titles similar to those that he himself had already made. Nor does it follow that the subject who gives the greatest number of correct judgments is the least suggestible. Subject A is more suggestible than subject D, although he has the greatest number of correct judgments. The other subjects, B and C, hold a middle place in regard to both correct judgments and suggestibility.

The results of this experiment seemed sufficiently encouraging to warrant the devising of a test along these lines. With this purpose fourteen of the best pictures have been selected.[1] The titles of these pictures have been approved by at least four of the five subjects. It is now the intention to test with them a larger number of subjects.

[1] These pictures can be obtained from the author, at cost, by anyone who desires to use them.

Psychophysical Symptoms of Deception

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PSYCHOPHYSICAL SYMPTOMS OF DECEPTION

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MANY treatises have already appeared upon the use of association-word reactions in detecting ideas or systems of ideas which are either consciously or unconsciously suppressed. The method has become well known particularly through the writings of Dr. Jung who has been primarily interested in bringing to consciousness the suppressed complexes which, according to his theory are the underlying cause of various abnormal states. This method of probing the subconscious by taking the time required to form association of ideas and by analyzing the word responses made to the stimulus word not only is one of the principle instruments of the psycho-analyst, but for some years has also been used as the basis of experiments aiming at the detection of deception. The method has on several occasions been used to detect actual crime, but for the most part such experiments have been performed under the artificial conditions of the laboratory in order to test the reliability of the results. The chief characteristic of the laboratory experiments is the selection of two individuals, one of whom is to carry out a series of acts of which the other is ignorant. The experimenter does not know which of the two individuals has committed the prearranged act, and his task is to discover the "culprit" by subjecting both individuals to the word-reaction experiment.

The details of the experiment have been clearly described by Professors Yerkes and Berry,[1] and are well known to all experimental psychologists. The method has in fact become sufficiently standardized to be used in training courses in laboratory technique.[2]

I have each year conducted the experiment both upon a group of men and upon a group of women. Latterly I have added the blood pressure test which has been developed by Mr. W. M. Marston and described by him in a paper entitled "Systolic Blood Pressure Symptoms of Deception."[3]

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[1]The Ass[illegible] Mental Diagnosis, Am. Jour. of Psychol., 1909, pp. 22-3[illegible]

[2]See [illegible] Laboratory Course [illegible]n Psychology," p[illegible] 112-116.

[3]Jou[illegible]

Notwithstanding the fact that an extensive literature has already appeared upon this subject it seems to me that certain of the conditions and results obtained this year in the experiment conducted in the course for women are of sufficient interest to justify a description of the experiment in some detail.

Twelve members of the class drew lots to decide which ones were to be the subjects in the experiment. This method of selection was used in order to avoid the criticism that subjects were intentionally chosen, whose temperaments were most conducive to a successful outcome of the test. It is obvious that if the guilty subject were of the highly emotional type and the innocent subject of the opposite type it would be easy to obtain positive results, especially from the blood pressure tests.

In this experiment Subject A was extremely nervous. During the test her cheeks were flushed and she moved restlessly in her chair. In fact she seemed to show all the outward signs of guilt, and before the results were examined the experimenters thought her guilty. In reality she was innocent. Subject B was of the more stolid type. She seemed very self-possessed, and her attitude was one of indifference such as would be assumed by the unemotional or highly controlled and successful deceiver. An objection frequently made to the practical application of this experiment is that a very nervous individual, although innocent, will be so disturbed emotionally by the mere fact of being examined that he will give incriminating responses. One of the most interesting features of the test is that this did not occur.

The two subjects selected were given their instructions and the description of the "crime" in sealed envelopes and were asked to leave the room together. Outside they tossed a coin to decide which was to carry out the crime. Care was taken that the innocent subject should know nothing of the nature of the crime. The subject whom chance designated as innocent opened the envelope which bore the legend "Direction to innocent subject," and obeyed the instructions which were as follows:

"Go into Room U and read a magazine which you will find on the table. When the guilty subject returns either you or she, as you two decide, will rap on the classroom door."

From Room U she could not see what was done by the other subject. The directions to the guilty subject read:

"Go into the Physical Laboratory in the Gilman Building (it is : large room at the end of the hall on the first floor). On the right you enter is a table used by the instructor, having four drawers and ›ors beneath. Open the left one of these doors and you will find on shelf the following articles:

1. A bottle of alcohol.
2. A bottle of coloring fluid.
3. An empty bottle with label and cork.
4. A typewritten letter with a stamped, addressed envelope and a sheet of blank paper attached.
5. A pencil.

"Examine all these articles carefully, reading the labels. Now our about one inch of alcohol into the empty bottle and fill almost ull of water at the tap. Put in enough coloring fluid to color the ıixture light brown, and shake thoroughly. Replace the alcohol and oloring fluid on the shelf.

"Now take the mixture you have prepared and wrap the bottle rith newspaper and string which you will find on the table. Copy the ypewritten letter in your own handwriting, place it in the addressed nvelope, and mail it in the box on Garden Street.

"After doing this take the parcel containing the mixture to Brown Tichols building. On the right as you enter the vestibule is a radiator. Conceal the package carefully on the floor behind the radiator. Detroy both this instruction sheet and the typewritten letter which you ave copied. Work quickly and secretly.

"Go now to Room U and join the innocent subject. Do not tell er or anyone else what you have done. You or she, as you two ecide, will then come and rap on the door of the class room."

The contents of the "typewritten letter" were:

Cambridge, Mass., 1921.

Dear Mr. Thurst:

Your order containing check for twenty dollars was received. You will find remedy No. 3 behind radiator in Brown Nichols. Date f manufacture was 1873. Container changed for protection.

Yours for further orders,

X. Y. Z.

The envelope provided for the mailing of this letter was addressed to Mr. A. B. Thurst, in care of the assistant in the course. The label on the bottle read "Scotch Rye Whiskey." One of the ottles contained in reality water and the other Worcestershire Sauce. The subject, however, did not know this. The crime and the details f carrying it out were arranged so as to make the situation as real s possible and to arouse in the subject a strong emotional reaction uch as would actually occur in one who had committed a misdemeanor.

It should be an act which the subject would hesitate to commit and which he would desire to conceal. In short, it should be sufficiently realistic to be highly suggestive of crime. Frequently in such experiments the subject is merely placed in the situation which will arouse the emotions, such as the handling of mice or the confrontation with a gruesome object; but in such instances there is an absence of the factor of concealment which is necessary for the true crime consciousness.[4]

When the instructions had been fulfilled one of the two subjects entered the classroom and was seated in a chair on the platform facing the class. The instructor sat next to her and gave the stimulus word to which she was instructed to respond with the first word which should come into her mind. The time was taken with a stop watch. There were fifty stimulus words, twenty-five of which—the *crucial* words—were related to the crime. The stimulus words were given in as quick succession as possible. When the list was completed the other subject was called and the same procedure repeated.

At the next meeting of the course three days later the blood pressure test was made. The subjects had been instructed not to discuss the experiment in the meantime nor to tell anyone which of them committed the crime. The subject was again seated on the platform. Mr. W. M. Marston, a member of the Massachusetts bar, cross-examined the subject and Mrs. Marston took the blood pressure. A Tycos Sphygmomanometer was used and the systolic pressure was recorded.

Before interrogating the subject a few blood pressure readings were taken in order to obtain a norm from which to calculate the rise in pressure due to the cross-examination. Such a norm consists of the normal blood pressure plus the pressure due to the excitement of the situation. The subject was then asked general questions not connected with the crime in order to get any possible rise due to the conversation. There then followed a period of cross-examination upon the events of the crime and finally a period of rest corresponding to the preliminary period. The blood pressure was taken during all these periods at an interval of about a minute.

RESULTS

In the table are the list of stimulus words and the reaction words and reaction times of the two subjects. The crucial words, that is, those connected with the crime, are in italics. The reaction times to

[4]For the details of the crime I am indebted to Dr. F. H. Allport who assisted me in the course.

the crucial words together with their deviations from the average are in separate columns from those to the non-crucial words.

The reaction times of the innocent Subject A to the crucial words were only on the average .37 seconds longer than the reaction times to the non-crucial words. This is 24% of her average reaction time to non-crucial words. The guilty Subject B, on the other hand, showed a difference of .83 seconds, or 62% of her average reaction time to the non-crucial words. Her delay in reacting to words connected with the crime was over twice that of Subject A. The amount of variation in the reaction times to the crucial words, as compared to the variation in the reaction times to non-crucial words, is frequently even more significant as an indication of guilt than is the difference in reaction times. In this instance Subject A showed a difference between the average variation of the crucial and non-crucial reaction times of only + 7 1/3 per cent while Subject B showed a difference of + 41 per cent. One is justified from a comparison of these figures alone in coming to a decision as to the guilt or innocence of the subjects.

In my experience with these tests I have found that a comparison of the reaction times and the average variations gave more valuable information than an analysis of the quality of the reaction words. It frequently happens that the innocent subject gives a word response that is closely related to the crime not because the subject is acquainted with any part of the crime, but because that particular association is a very common one to make with the stimulus word. Any judgment, therefore, based on such a response would be misleading. For example, Subject A responded with "rum" to "bottle," with "Scotch" to "whiskey" and with "postman" to "mail," etc. On the other hand Subject B's response of "alcohol" to "whiskey" cannot in itself be considered suspicious. In several experiments where the judgment was based upon the quality of the reaction words it was incorrect, while in the many experiments I have made in which the judgment was based upon a decided difference in reaction times and mean variations, the right subject was judged guilty.

Attention should also be called to the fact that in the case of the innocent subject several complexes of a private nature in no way connected with the crime were set off, once by the non-crucial stimulus word "morphine," another time by the crucial word "destroy" and a third time by the crucial word "Gilman." The word "destroy" was associated in the student's mind with a recent fire in the dormitory

which destroyed most of her property.[5] It happened in this instance that the lengthened reaction times practically cancelled each other. Such a cancellation, however, is not likely to occur when only fifty stimulus words are used. On account of the element of chance in tapping such extraneous complexes it is advisable, if possible, to use several hunderd stimulus words.

In charts I and II are plotted the rise and fall of blood pressure of the two subjects. As Mr. Marston has shown in the above cited paper, the significant feature in the lying curve is the difference between the maximum rise in the curve during the lying period and the average blood pressure during the pre-lying period. The characteristic lying curve shows a gradual rise in pressure during lying with a fall in the post-lying period, the latter being due to the release from the tension of the lying period when the subject realizes that the ordeal is over. This is the form of the curve for the guilty Subject B. The difference between the maximum rise in the lying period and the average pressure during the pre-lying period for Subject B is + 24 mm. as compared with only + 7 mm. for Subject A. This is of special interest from the fact stated above that Subject A was of the very nervous type, and it might have been supposed that she would show considerable rise in pressure due to the excitement which was very evident to anyone observing her. Subject B had herself well under control, but the suppression of the crucial facts influenced the blood pressure in an unequivocal manner. Her maximum rise occured when she denied that she had been in Gilman Hall.

Turning to Subject A's curve we find that there is a drop instead of a rise at A when she was asked if she had been in Gilman Hall, another at B when she was asked about the bottle, and again at C when questioned about the coloring fluid. There was nothing in the situation to cause in Subject A a feeling of relief in the post-period. Her curve in fact rises, due, as she informed us, to her anticipation of the verdict.

An additional fact that throws light upon this problem of blood pressure changes is that Subject A actually lied on several occasions during the cross-examination. The lies, however, had no special significance. They were not connected with the situation, nor did she care whether they were believed or not. In fact they were such obvious lies that she must have known that they would be detected by

[5]As so frequently happens the lengthening of the reaction time did not occur until the stimulus word which succeeded the complex-arousing word was given.

all those present. For instance she said she had not been at the last meeting of the class. Since she had taken part in the experiment at the last meeting the mis-statement was very evident to the audience. There was, therefore, no suppression upon her part, and consequently no effect on the blood pressure.

SUMMARY

The following conclusions, although drawn from only one test, are offered as suggestive:

1. The average reaction time of the guilty subject to the crucial words of the test was considerably larger than the average reaction time to the non-crucial words. The mean variation of the former was larger than that of the latter. This occurred in spite of the fact that the guilty subject was of the restrained and controlled type, and so far as outward appearances are concerned, made every effort to deceive.

2. In the word reaction test for detecting deception the reaction time and mean variation are more reliable factors than a qualitative analysis of the reaction words. This conclusion is based not alone upon the results of this experiment, but upon those of a series of tests conducted each year in the class room.

3. The innocent subject was much more nervous than the guilty subject. Nevertheless during the cross-examination the blood pressure of the guilty subject rose considerably higher than that of the innocent subject. This result answers the frequent criticism of the test that a nervous witness under cross-examination will show the physical symptoms of guilt.

4. The innocent subject told several lies during the blood pressure test. She knew, however, that the prevarications were obvious to her auditors. The fact that these lies did not cause the characteristic rise in blood pressure seems to support the assumption that suppression, which is an essential part of the deception consciousness, is a cause of the rise.

Subject A

Reaction Word	Reaction Time		m. v.		Stimulus Wo
	Non-Cru.	Cru.	Non-Cru.	Cru.	
win		.7		1.17	*whiskey*
play	1.		.5		theatre
dog	.9		.6		drag
run		1.3		.57	*bottle*
bottle		1.3		.57	*label*
beach		1.6		.57	*rye*
architect	1.7		.2		build
church	1.1		.4		ceremony
egg	1.1		.4		chicken
bottle		1.2		.67	*container*
clock		1.2		.67	*shelf*
whiskey		1.2		.67	*Scotch*
dress	.9		.6		pattern
apple	1.1		.4		orchard
ink	.9		.6		blotter
telepathy	1.7		.2		mental
gold	1.2		.3		silver
dress	1.1		.4		hat
mining	1.4		.1		industry
wood		1.4		.47	*alcohol*
fire		1.9		.03	*destroy*
goods		4.3		2.43	*manufacture*
postman		2.2		.33	*mail*
philosophy	1.9		.4		textbook
knife	1.6		.1		sharpen
money	1.3		.2		fortune
blue		1.5		.37	*coloring*
sulphuric acid.		1.6		.27	*dilute*
dog		1.8		.07	*shake*
house	1.2		.3		Agassiz
mountain	1.2		.3		climb
watch		5.1		3.23	*Gilman*
training		1.6		.27	*physical*
mail		1.8		.07	*letter*
train	1.7		.2		break
appendicitis ...	1.3		.2		operation
engine	1.6		.1		accident
mother	1.2		.3		children
physician		2.1		.23	*remedy*
water		1.4		.47	*thirst*
tree	1.7		.2		flower
ceremony	1.4		.1		church
dope	3.2		1.7		morphine
car	4.3		2.8		automobile
intoxicated ...		2.		.13	*liquor*
magazine		2.1		.23	*newspaper*
death		1.8		.07	*poison*
Radcliffe		2.3		.43	*Brown Nicho*
heat		1.5		.37	*radiator*
hide		1.8		.07	*conceal*
Average	1.50	1.87	.464	.575	

	Subject
Crucial Difference (av. cru. R. T.—av. non-cru. R. T.) =	+ .37
Crucial Difference expressed in per cent. of av. non-cru. R. T. =	24
Mean Variation Difference (m. v. of cru.—m. v. of non-cru. R. T.) =	+.11
Mean Variation Difference expressed as per cent. of non-cru R. T. =	7⅓

Subject B

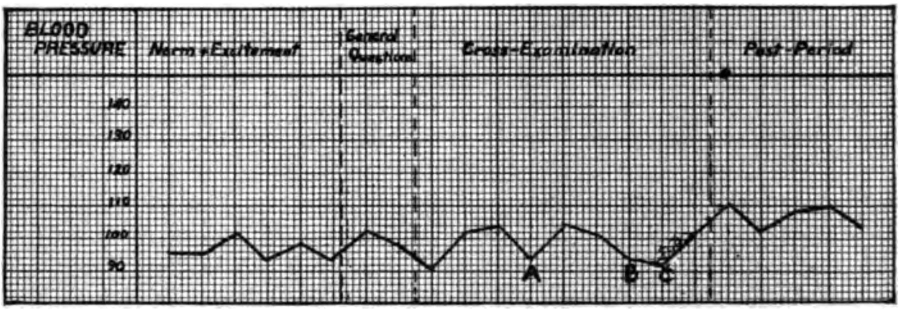
Reaction Word	Reaction Time Non-Cru.	Reaction Time Cru	m. v. Non-Cru.	m. v. Cru
alcohol		.2		.07
people	1.4		.21	
net	1.1		.09	
cork		1.2		.73
paper		1.4		.53
wheat		1.3		.63
house	1.1		.09	
minister	1.1		.09	
rooster	1.1		.09	
tin		1.3		.63
wood		1.3		.63
English		1.3		.63
paper	1.1		.09	
tree	1.2		.01	
paper	1.2		.01	
test	1.2		.01	
dollar	1.1		.09	
coat	1.1		.09	
busy	1.3		.11	
whiskey		1.5		.43
paper		1.6		.33
articles		2.5		.57
female		1.2		.73
Langfeld	2.2		.29	
knife	.9		.19	
teller	1.		1.01	
ochre		1.9		.03
ink		3.4		1.47
bottle		2.5		.57
house	1.2		.01	
tree	.9		.29	
street		2.1		.17
chemistry		1.4		.53
write		1.6		.33
stone	1.1		.09	
moving picture.	1.1		.09	
car	1.5		.31	
grown-up	1.4		.21	
doctor		1.6		.33
hunger		1.1		.83
garden	1.		.19	
minister	1.2		.01	
opium	1.		.19	
chauffeur	1.3		.11	
bottle		5.9		3.97
print		1.8		.13
iodine		3.		1.07
Garden St. ...		1.8		.13
heat		2.1		.17
hide		1.4		.53
Average	1.19	1.93	.159	.646

m. v. = mean variation
non. cru. = non crucial
cru. = crucial
R. T. = reaction time

	Subject B
Crucial Difference (av. cru. R. T.—av. non-cru. R. T.) =	+ .83
Crucial Difference expressed in per cent. of av. non-cru. R. T. =	62%
Mean Variation Difference (m. v. of cru.—m. v. of non-cru. R. T.) =	+ .487
Mean Variation Difference expressed as per cent. of non-cru. R. T. =	41%

CHART I

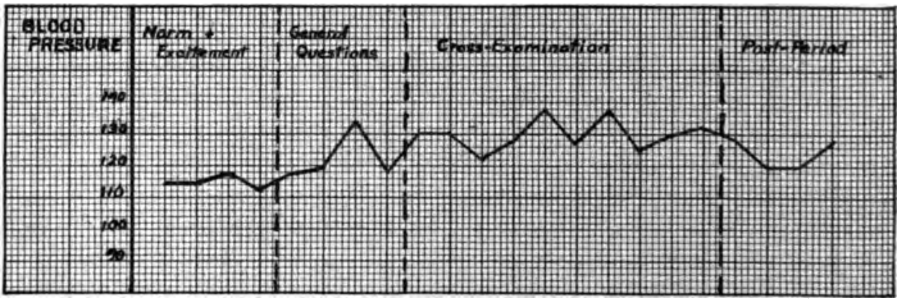
Subject A *Blood Pressure*



Average norm excitement	97 mm
Maximum rise during cross-examination	104 mm
Rise	7 mm

CHART II

Subject B *Blood Pressure*



Average norm excitement	114 mm
Maximum rise during cross-examination	138 mm
Rise	24 mm

THE AFFECTIVE TONE OF LINES: EXPERIMENTAL RESEARCHES[1]

BY HELGE LUNDHOLM

Literature about Art very often gives us descriptions of masterpieces, wherein pure lines are characterized by adjectives that indicate a more or less emotional quality. Thus authors used to write about melancholy lines in paintings by Perugino, quiet lines in certain classical schools, violent lines in the barocque art, etc. Out of these facts there arises a problem. Is the affective character of the line a quality which is bound to the line itself, or is it suggested by the literary subject of the masterpiece? Furthermore, is this quality a phenomenon that appears equally to different observers? In order to throw light upon these questions, the following experiments were undertaken. The procedure was very simple. The persons who acted as subjects were asked to draw lines, each of which was to express the affective tone of an adjective given verbally. In the first series 48 adjectives, divided into 13 groups of synonyms, were used as follows:

I. Sad, melancholy, mournful, doleful, sorrowful.
II. Quiet, calm, tranquil, serene.
III. Lazy, indolent, idle.
IV. Merry, cheerful, gay, jolly, joyous.
V. Agitating, exciting, sprightly, fiery, brisk, vivacious, lively.
VI. Furious, angry, cross, vexed, enraged.
VII. Dead, dull.
VIII. Playful.
IX. Weak, feeble, faint, delicate.
X. Gentle, mild.
XI. Hard, harsh, cruel.
XII. Serious, solemn, grave, earnest.
XIII. Powerful, forceful, strong.

[1] From the Psychological Laboratory of Harvard University, 1919–1920.

Two other series followed: the first of these was to find out if certain lines, so to speak, accord with certain colors; the second to learn what is characteristic of purely beautiful lines. The adjectives of these series were:

XIV. Red, blue.

XV. Beautiful, ugly.

Each line was drawn with a pencil on a sheet of white paper, 21 × 27 cm. in size. The greatest liberty was allowed the subjects even with regard to the time used in drawing the lines. The manner of moving the hand and pencil while drawing was carefully observed and described in each individual case. No record was taken either of the pressure of the pencil or of the time used in drawing each line. The series were repeated, the only change in the instruction being that the line should be drawn as rapidly as possible. The first and second line of each type were compared and if they differed the subject was requested to draw a new one, after which he decided which of the lines most satisfactorily expressed the purport of the adjective in question. It sometimes happened in the first series, that a subject drew several lines of one and the same type as if for trial. In those cases too they decided themselves which line satisfied them most fully. In tabulating the results only one line of each type was used and then always the one which had been judged most expressive.

The subjects were requested to express the adjective as far as possible by a pure line, not to symbolize sadness by the curve of a melancholy mouth or strength by a line suggesting the contour of a rock-formation, etc. Yet they were earnestly instructed to mention associations of this nature. Furthermore they were requested to describe the relation of every synonym to the first adjective in the group, in order that the experimentor might know what meaning the subject attached to the various adjectives. The introspection was carefully noted, and as far as possible verbatim. The sequence of the lines was such that two lines belonging to the same group never followed each other and were very seldom drawn in the same period.

The subjects were eight in number: four women, G, L, Mi, S; and four men, C, D, F and Ma. No one of them was either painter, or designer, nor did they know anything about the theories of the æsthetics of lines.

The experimentation took place from October 1919 to May 1920 in the Psychological Laboratory of Harvard University.

When the experiments were finished an examination of the results revealed certain principles by which the lines with a few exceptions could be classified. Thus they could first be separated into the following groups:

I. Lines with only curves.
II. Lines with only angles.
III. Lines with both angles and curves.

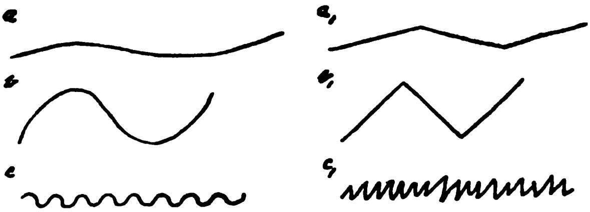


FIG. 1.

The first group (I) could in turn be divided into three subdivisions:

(1) Lines with a few long and low waves (Fig. 1, *a*).
(2) Lines with a few high waves of medium length (Fig. 1, *b*).
(3) Lines with numerous small waves of varying shape (Fig. 1, *c*).

Correspondingly the second group (II) could be subdivided into:

(1) Lines with a few obtuse angles (Fig. 1, a_1).
(2) Lines with a few approximate right angles (Fig. 1, b_1).
(3) Lines with numerous acute angles (Fig. 1, c_1).

Finally the third group (III) could be subdivided according to the way in which waves of type *a*, *b*, and *c* were present in combination with angles of type a_1, b_1, and c_1.

Another point of view from which the lines could be classified was that concerning their general direction. They

were supposed to be drawn horizontally from left to right in the largest dimension of the paper. Nevertheless in some of them there was a marked tendency to run upwards, in others a tendency to run downwards. These two together with the horizontal direction gives us three more groups.

It is self-evident that a classification of such a great number of lines as the experiments produced, must always be, to a certain extent, approximate. As has already been said the small waves were very often of a varying shape, and this variation grew still greater when they occurred in combination with angles. The latter in turn, especially acute angles, also showed very varying forms. Likewise there were found intermediate forms which were placed in the system according to the degree with which they approached the main types. It was easier to group those lines which had long and low waves and obtuse angles. Waves of type *a* very seldom occurred together with waves of type *c* in one and the same line. In a few cases waves of the latter type were found mixed with those of type *b*. In such cases the lines were placed within sub-group (2) or (3) according to the frequency of the different waves. Quite analogous was the situation regarding the occurrence of angles of the types a_1, b_1, and c_1.

The long and low waves and the waves of medium length were very few in number in each line, as a rule there were only 1½ or 2 of them; while the small waves were numerous, in most cases 15 or 20. In the same way the obtuse and right angles always occurred less frequently than the acute ones. Sometimes small waves and acute angles were found as a secondary oscillation of a long wave or of a wave of medium length. This was often the case when the main direction of a line had an upward or downward tendency. Since in such cases the introspection did not attribute any other importance to the main wave than that of indicating upward or downward movement, these lines were always grouped under the type of small waves and acute angles.

On the following pages the lines are tabulated according to the two points of view mentioned. The headings of the

TABLE I.

	A			*C*			*A* and *C*						*Di*		
							A			*C*					
	B	*M*	*S*	*B*	*M*	*S*	*B*	*M*	*S*	*B*	*M*	*S*	*H*	*U*	*D*
I.															
(*a*) Sad........				7	1								2		6
(*b*) Melancholy.				7	1								1		7
(*c*) Mournful...				6	1	1							1		7
(*d*) Doleful.....				5	1				1			1	1		6 Ma°
(*e*) Sorrowful...				7		1								1	7
Total.....				32	4	2			1			1	5	1	33
II.															
(*a*) Quiet.......				8									7		1
(*b*) Calm.......				8									5		3
(*c*) Tranquil....				8									6	1	1
(*d*) Serene......				8									8		
Total.....				32									26	1	5
III.															
(*a*) Lazy.......				8									1		7
(*b*) Indolent....				7	1								3		5
(*c*) Idle........				7									3	1	3 C°
Total.....				22	1								7	1	15
IV.															
(*a*) Merry......						6			2		2		4	4	
(*b*) Cheerful....				2	1	3		1	1	1	1		3	5	
(*c*) Gay........					1	5	1		1	1	1		3	5	
(*d*) Jolly.......					3	3			2		2		5	3	
(*e*) Joyous.....				1	2	3			2		2		1	6	1
Total.....				3	7	20	1	1	8	2	8		16	23	1
V.															
(*a*) Agitating...		3							5		5		4	2	2
(*b*) Exciting....				1		2			5		5		3	5	
(*c*) Sprightly...						2			6		6		7	1	
(*d*) Fiery.......		3							5		5		5	2	1
(*e*) Brisk.......		2						1	5	1	5		4	4	
(*f*) Vivacious...					1				6	1	5		2	6	Ma
(*g*) Lively......									7	1	6		4	3	1 Ma
Total.....		8		1	1	4		1	39	3	37		29	23	4
VI.															
(*a*) Furious.....			2						6		1	5	4	3	1
(*b*) Angry......			1						6			6	4	2	1 S
(*c*) Cross.......			2	1					4		1	3	3	2	2 G°
(*d*) Vexed......			2				1	1	4		1	5	3	4	
(*e*) Enraged....			1			1			5			5	3	4	1 D
Total.....			8	1		1	1	1	25		3	24	17	15	5

	A			C			A and C						Di		
							A			C					
	B	M	S	B	M	S	B	M	S	B	M	S	H	U	D
VII.															
(a) Dead.......		1		5									7		1 D.L.
(b) Dull........	1			6									5		3 D.
Total.....	1	1		11									12		4
VIII.															
(a) Playful.....					1	2			4			4	3	4	1 S
IX.															
(a) Weak......				4		4							3		5
(b) Feeble......				3									3	1	4 Mi, L, C, G, Ma.
(c) Faint.......				5	1								3		5 S.F.
(d) Delicate....				1	1	4			1			1	5	2	1 Ma.
Total.....				13	2	8			1			1	14	3	15
X.															
(a) Gentle......				7		1							5		3
(b) Mild.......				5	1	1							7		1 Ma
Total.....				12	1	2							12		4
XI.															
(a) Hard.......	1	5	2										6	1	1
(b) Harsh......			4						4		1	3	3	1	2 Ma, S.
(c) Cruel.......		1	5		1				1			1	4	2	2
Total.....	1	6	11		1				5		1	4	13	4	5
XII.															
(a) Serious.....				5	3								4	1	3
(b) Solemn.....				6		1							5		3 Ma
(c) Grave......				6	2								3	1	4
(d) Earnest.....				5	1									4	4 S Ma
Total.....				22	6	1							12	6	14
XIII.															
(a) Powerful....		1		3	4								5	3	
(b) Forceful....	1	2	1	2	1				1			1	3	4	1
(c) Strong......		2		1	4	1							1	5	2
Total.....	1	5	1	6	9	1			1			1	9	12	3

columns mean: (*A*) angles, and (*C*) curves, (*B*), (*M*), and (*S*), big, medium, small, referring respectively to waves of the types Fig. 1, *a*, *b*, and *c*, and angles of the types Fig. 1, a_1, b_1, and c_1. (*Di*) means chief direction, (*H*), (*U*), and (*D*), horizontal, upward tendency and downward tendency. On

the right side of each table are indicated lines which for one reason or another are outside of the groups. The letters at the right indicate the subjects who draw those unclassified lines, while the horizontal line in which they stand indicate the type of the line (sad, joyous, etc.). A letter with index ° means that the line was not drawn by the subject.

Table II. shows how the lines are divided if each group of synonyms is taken as a whole. The numbers are reduced to a per cent. of the total number, within the group.

TABLE II.

	A			*C*			*A* and *C*						*Di*			
							A			*C*						
	B	*M*	*S*	*B*	*M*	*S*	*B*	*M*	*S*	*B*	*M*	*S*	*H*	*U*	*D*	
I. Sad, etc.				82	10	5			3			3	13	3	84	
II. Quiet, etc.				100									81	3	16	
III. Lazy, etc.				92									29	4	63	4
IV. Merry, etc.				8.4	18	50	2	2	20		4	20	40	58	2	
V. Agitating, etc.			14	2	2	7		2	70		5	67	52	41	7	3
VI. Furious, etc.			21	2.5		2.5	2.5	2.5	64		8	61	44	38	13	5
VII. Dead, etc.	6	6		69									75		25	19
VIII. Playful					13	26			50			50	38	50	12	11
IX. Weak, etc.				41	6	25			3			3	44	9	47	25
X. Gentle, etc.				75	6	13							75		25	6
XI. Hard, etc.	4	25	46		4				21		4	17	54	17	21	8
XII. Serious, etc.				69	19	3							38	18	44	9
XIII. Powerful, etc.	4	21	4	25	38	4			4			4	37	50	13	

If, to begin with, we look over the first six groups in the tables we find that in group I.–III. (sad, quiet, lazy, etc.) most of the lines show curves of type *a*, *i.e.*, 82 per cent., 100 per cent. and 92 per cent., while the number of lines drawn with small curves, and with angles and curves is very inconsiderable. In the groups IV.–VI. (merry, agitating, furious, etc.), which contain adjectives indicating to a certain extent opposite mental states, the contrary is true, that is, a very great number of lines show short waves and acute angles while waves of types *a* and *b*, and angles of a_1 and b_1, are a decided minority. It is also striking that in Group IV. (merry, etc.) lines with only short waves of type *c* predominate and that in Group V. (agitating, etc.) we meet for the first time lines with only angles, and those all of type c_1.

Hence we find a relative uniformity in the lines belonging to groups I.–III. (sad, quiet, lazy, etc.) as well as a certain uniformity in the lines of groups IV.–VI.(merry, agitating, furious, etc.). If we compare these facts with the qualities suggested by the corresponding adjectives we find that all those adjectives indicating a state of mind of little motor expression (groups I.-III.) have been symbolized by lines consisting chiefly of long and low waves, while those which indicate states of strong motor expression have been symbolized by lines of either short waves alone, or acute angles alone, or of both. This result can be illustrated by the following formula, in which *m* means states of mind with little, *M* with strong, motor expression and the other letters refer to the different types of waves and angles already described.

$$\text{I.} \quad m = a \qquad\qquad \text{II.} \quad M = \begin{cases} c \\ c_1 \\ c + c_1 \end{cases}$$

It has long been known that pure lines in themselves are able to suggest movements or motor states. Consequently the question arises whether there is a law for these phenomena; that is, whether certain lines suggest movement in a higher degree than others. An answer to this question was obtained by the following simple experiments. Each of the subjects was asked to draw four wave lines suggesting movements of different degrees of intensity.[1] They all drew them alike. The line of least movement had long waves of type *a*, the next had shorter waves, and compared with the length, higher waves, the third had still shorter waves and the fourth small waves of about the same type as *c*. It is to be remarked that in the last line the waves were frequently sharpened and approached acute angles. These experiments show, undoubtedly, that long and low waves suggest less movement than shorter and higher ones, and furthermore that the movement suggested by a line becomes accentuated if this contains acute angles. Several introspective statements of the subjects verify these results. A few of them follow:

[1] By intensity is meant both rapidity and the force which is read into the line.

G: There is often much movement in an angular line but this movement is of a jagged, broken, and hard character.

L: Small waves make the movement of a line go more quickly. The calm line has slow, long curves.

Mi: Sharp angles give a sensation of speed—much action. Broad curves represent more enduring emotions, while the small curves show more transitory and volatile emotions. They are more rapid.

C: Angularity of a line expresses violence of movement. If a curve is slowly undulating or sweeping it is soft. To be so, it must have long waves with low amplitude. Angles and straight lines always express violence, energy of movement. A long curve always expresses slowness.

F: Angles, especially the sharp ones, express vivacity. Short curves imply vivacity.

Many more statements of the same nature could be quoted, but those already chosen are sufficient, especially since there is not a single contradiction.

Hence we see first that lines symbolizing states of strong motor expression have short waves and acute angles and lines symbolizing states of weak motor expression have long and low waves; and second that lines with waves of the former type and acute angles themselves suggest intense motion, while lines with waves of the latter type suggest weak and slow motion. This justifies us in supposing that the affective character of lines has its origin in the suggestion of movement of the line that it depends upon the idea that this movement in some way imitates the motor expression of an emotion. This supposition becomes greatly strengthened by the fact that the subjects themselves have mentioned the movement as being of importance for the emotional expression of the lines.

How the illusion of movement itself in the pure line arises, is another problem which has been treated to a great extent in psychological literature. The only light that was thrown upon the theory of the phenomenon by the subjects was, that three of them agreed in explaining that the suggestion of movement of lines was connected with an idea of the motion of the hand in drawing them. One of the subjects connected the phenomenon with the movement of the eye in following the line; the rest did not express any opinion.

If we compare groups IV. (merry, etc.) and VI. (furious,

etc.) in the tables, we find that in the former 50 per cent. of the lines show only small waves, and 20 per cent. show small waves and acute angles, while in the latter only 2.5 per cent. show small waves, and 64 per cent. small waves and acute angles. There is an obvious difference between the two groups, although both of their adjectives indicate states of strong motor expression. We obtain the explanation of this, however, if we examine the general introspective statements regarding the feeling tone of curves and angles, which were given during the progress of the experiments. A complete and systematical quotation of them follows:

G: Sharp angles are unpleasant—weakness can never be expressed through angles. The rapid interruption through angles gives the impression of furiosity. Angularity of a line suggests sharpness, impatience, hard-heartedness, a certain unfeeling vigor and strength. Likewise angularity implies absence of gentleness and grace. Very little refinement can be suggested by it. There is often very much movement in it, but of a jagged, broken and hard sort. It lacks high purposes; it is an unstudied line of least resistance.

Curves suggest grace, serenity and most of those physical and mental qualities acquired in civilization and education. The curving of a line gives it more maturity, it gives the poise and refinement of nature. It does not lack strength, it always expresses a high and rather moral quality of a feeling.

L: Sharp angles hurt. Angularity expresses the hard, angry, and unpleasant emotions. It indicates fire, storm, strength and power.

Curves denote grace and beauty, serenity and kindness. The finer characteristics of all elements are shown by curves. Big curves express graveness, firmness and strength.

Mi: Small angles are cruel because they remind one of stabbing points. Angularity of a line usually expresses ungracefulness, too much expenditure of energy for the execution of the intended movement. Much action. Suggests the emotion of anger, fear, surprise, excitement, all emotions which are sudden and immediate. Also expresses pain, anguish, brutality and sharpness. Obtuse angles represent emotions of a slower, less emphatic sort; sharp angles give a sensation of speed and deeper excitement and a higher point of emotional instability.

A curving line usually represents grace and economy of movement. It is pleasanter than an angular line, because it does not take so much trouble to follow it. It suggests gentle emotions, such as pleasure, happiness, gaiety, and the opposite sorrow and grief. Broad curves are more graceful than small curves and represent more enduring emotions, while the small curves show more transitory and volatile emotions. Power is expressed in big curves, even dignity.

S: Angularity means awkwardness and hardness, lack of gracefulness in the movement. I do not think a right angle could ever be soft. Could not connect an angular line with gentleness, because it is hard and sharp.

Curving of a line means sweeping motion. Anything curved, going very slowly, is mild.

C: Angles give the idea of violence and vivaciousness; they imply viciousness, anger, etc. There is a correlation between straight lines and angles, and hardness.

A long curve always expresses smoothness and slowness. Lines curving upwards express increase of *tonus*, lines curving downwards express relaxation—sometimes depression. A curve is soft if it has long, low waves which undulate in a smooth way.

D: Harshness must always be expressed with angles. The weak line has not energy enough to make angles. Angles express the absence of conscious idea. Angles in a line imply the more abrupt and violent emotional states. They represent feelings that are more or less spasmodic in expression with no special inhibition of a tendency towards marked accent. Angles are harsh and consequently striking. Angles, on the whole, express the more intense, elementary feelings.

In contrast to angles, curves in a line usually represent the more refined and purely intellectual feelings. They tend to modify and control even the most violent of emotional states and consequently are usually the more pleasing of the two. Curves also tend towards a more intimate coördination between the different elements expressed in a feeling state; and hence aid in uniform and consistent expression of any particular state or states. Curves appeal to the finer and more highly differentiated aspect of the subject.

F: Angular motion represents hard and painful feelings. Angles even tend to express strength, vivacity—sharp angles, distress; broader angles, power, determination, calmness.

Curves express grace and are usually pleasant. Short curves, grace and liveliness; long curves, beauty, indolence, calmness. Curves in general express the weaker and less forceful emotions.

Ma: Sharp angles imply the idea of pain, pricking pain, spitefulness, incongruity, instability, moodiness. Angles even imply sharpness and sudden transition, brusqueness, caustic feeling, quick temper, ugliness.

Curves imply gradual transition, the more subtle emotions, prettiness, lack of much strength, smoothness.

With the above-quoted introspection as a back-ground we can easily understand the difference in character of the lines belonging to groups IV. (merry, etc.) and VI. (furious, etc.); it is the general pleasantness of the emotions in the former group which has caused the subjects to symbolize these emotions with chiefly curved lines, while it is the unpleasant and unrefined feeling tone of the emotions in the latter group, that underlies the use of so many angles in its lines. We can likewise understand the distribution of angles and curves in groups IX. (weak, etc.) and X. (gentle, etc.) on one hand and in group XI. (hard, etc.) on the other. In the first two of these groups the curves are in a decided majority (72 per cent. and 94 per cent.) and in the latter the angles (75 per cent.) and the angles and curves (21 per cent.). The reason for the long and low waves predominating in

groups IX. and X. is without doubt that the state indicated has a weak motor expression, while the reason for the predominance of acute angles in group XI. is due to the more painful feeling tone connected with the character of the states expressed through its adjectives.

If we compare groups IX. (weak, etc.) and XIII. (powerful, etc.) which are opposite in regard to the meaning of their adjectives, it will be noticed that in the former, curves of type *b* are in a decided minority while in the latter, the same curves together with angles of type b_1 tend to predominate. These groups consequently show first in a negative and secondly in a positive sense, that high curves of medium length are the ones which above others suggest strength. Group XIII. also seems to imply that the right angles of type b_1 possess the same quality. A few introspective statements already quoted in another connection verify this.

L: Big curves express graveness, firmness, and strength.
Mi: Power is expressed in big curves, even dignity.
S: I do not think a right angle could ever be soft.
F: Broader (in opposition to sharp) angles represent power.

Finally if we examine those groups not yet treated, VII. (dead, etc.) VIII. (playful) and XII. (serious, etc.), we can easily interpret their lines from the points of view which we have already gained. VII. and XII. contain chiefly waves of type *a* (69 per cent. and 69 per cent.), obviously depending upon the inactive character of their adjectives; VIII., on the other hand, the more active group, contains chiefly waves and angles of types *c* and c_1 (50 per cent.) or waves of type *c* only (26 per cent.). Perhaps we should have expected the last number to be a little larger considering the pleasant feeling tone of the state.

Those lines which differ too widely from our categories to be subordinated to any one of them, show few traits of interest. As a rule their shapes have been fixed by one or another concrete association. In only a few cases can the deviating lines be said to form a category of their own, as for instance in group VII. (dead, etc.), where they are all horizontal straight lines, or in group IX. (weak, etc.), where a common

quality in seven of them is that they are each drawn with a consciously trembling hand. The association lying behind this is too obvious to need any description. That straight horizontal lines have been used as symbols for death and dullness has, according to the statements of the subjects themselves, its cause in their lack of motion.

Before we proceed further it mght be useful to incorporate some of the new results in the formulæ which we have already drawn up. This can be done by dividing the second of them into two new ones. Thus:

$$\text{III.} \qquad Mp = \begin{cases} c \\ c_1 \\ c + c_1 \end{cases} \qquad c > c_1$$

$$Mu = \begin{cases} c \\ c_1 \\ c + c_1 \end{cases} \qquad c_1 > c$$

Mp means an emotion with strong motor expression and pleasant feeling tone. *Mu* an emotion with strong motor expression and unpleasant feeling tone. $>$ means more frequent than.

As already stated, the lines could also be divided into groups according to their chief direction, which is recorded in the last columns of the tables. In regard to these also, general tendencies can be established.

If we begin by giving our attention to groups I. (sad, etc.) and IV. (merry, etc.) we find that in the former 13 per cent. of the lines are horizontal, 3 per cent. inclining upwards and 84 per cent. inclining downwards, while in the latter 40 per cent. are horizontal, 58 per cent. inclining upwards and only 2 per cent. inclining downwards. The explanation is given by the introspection of the subjects, in the following statements:

The downward tendency of a line expresses relaxation, the upward expresses power. The downward tendency expresses faintness, not sufficient strength to keep up. Going downwards expresses losing of energy. The doleful line droops without energy. If it had force it would have ascended

higher. Strength is expressed by going upwards. A joyous line also ascends. Joy is an uplifting feeling. A forceful line tends upwards. Thereby it obtains the idea of ambition. A line indicating strength is a line tending upwards, never downwards.

On not less than 57 different occasions analogous statements have been noted, and not a single one shows a tendency towards an opposite opinion.

Therefore it seems to be obvious that even the direction of the lines to a certain extent imitates the motor expression of an emotional state and that consequently the direction is one of the factors that partakes in giving them their affective tone. Direction upwards expresses strength, energy, force, ambition, uplifting feelings, etc., direction downwards, weakness, lack of energy, relaxation, depression, etc.

If we look over the different groups with attention to their chief direction, this is verified. In the groups IV., V., VI., VIII. and XIII. containing synonyms for merry, agitating, furious, playful, and powerful we find more upward than downward tending lines, while in I., II., III., VII., IX., X., XI. and XII. groups, containing synonyms for sad, quiet, lazy, dead, weak, gentle, hard, and serious, a larger number of lines tends downwards than upwards. All these facts correspond to the general differences in the emotional states so far as the qualities just mentioned are concerned.

It is interesting to note that in group II. (quiet and its synonyms) namely the indifferent group, the largest per cent. (81 per cent.) of the lines tends towards the horizontal. Why group XI. (hard and its synonyms) has more downward than upward-tending lines can not be explained through the introspection directly. The differences in per cent. (21 per cent. to 17 per cent.) is small enough, however, to make it possible that it may be due to chance.

The experiments were not carried on in such a way that they gave exact records either of the fashion in which the lines were drawn, or the time used in drawing them. Investigations concerning these could be undertaken and should give interesting results. Judging from the observations made

by the experimenter a tendency seems to exist to draw the more rapidly, the stronger the motor expression of the state of mind to be symbolized is. Thus as a rule the joyous lines were drawn much more rapidly than the sad ones. Likewise the small waves and the acute angles were generally drawn more rapidly than the big waves and the broad angles. There were exceptions, as for instance when cheerfulness was expressed through one single big upward-going half wave, drawn with great rapidity, or when excitement was expressed in an analogous way. Even the thickness of the line and the pressure of the pencil seemed to follow certain rules. Strength was very often expressed by a homogeneous thickness of the line caused by a strong continuous pressure of the hand when drawing. We have several similar introspective statements. Breadth of a line gives the impression of strength. It expresses intensity, strength and great saturation.

On the contrary, lines belonging to the group of "weak" and its synonyms are drawn as a rule so thinly that at times they can hardly be seen. One of the subjects even defined a weak line as a line that "you hardly can see." Such observations indicate that further experiments aiming to record the rate and manner of drawing the lines would be worth while.

As has already been said, the subjects were requested to avoid concrete associations as much as possible, but at the same time to mention if they happened to appear. Few associations have been noted. This, however, does not exclude the fact that such did influence unconsciously the shape of the lines, and their feeling-tone. The following is a complete record of all associations written down in the subjects' own words:

G: Sad: drooping lips, gloomy valley pits. Indolent: picturesque laziness of southern people, vagabond. Merry: childish quality, like jumping. Jolly: like a kitten. Joyous: bumping movement like a ball; a boy turning somersaults. Vivacious: harlequin. Fiery: dragon. Playful: a cat playing with a ball. Hard: undecorated wall. Strengthful: pyramids and mountains.

L: Cheerful: something rippling. Joyous: dancing. Furious: sharp points that hurt.

Mi: Merry: the country, jumping. Fiery: flames in a fire. Weak: a weak chin. Hard: saw teeth. Powerful: buildings and big waves.

S: Melancholy: a weeping willow tree. Doleful: a doleful face. Merry: making noise. Joyous: child clapping its hands.

C: Indolent: a yawn. Merry: dance. Furious: lightning; musical associations. Sprightly: movements of a danseuse.

D: Fiery: lightning.

F: Idle: idea of walking around. Vexed: angles represent a hard and painful feeling.

Ma: Sad: a man suddenly starting up and then relaxing. Lazy: the general outline of a slope. Idle: a man doing nothing. Merry: turning round, jumping. Cheerful: dancing. Dull: a dull knife. Weak: a weak mouth. Hard: the rock of Gibraltar. Serene: the idea of thought. Serious: sort of a brain wave.

The few investigations made in order to find out whether pure color-images could be symbolized with lines show that it is chiefly the exciting and quieting elements in the colors which induce the shape of the lines. The different types of lines which we have separated are divided in the following way:

Red: $c_1 = 3,\ a = 1,\ c = 1,\ c + c_1 = 2$
$H = 2,\ U = 2,\ D = 2.$

Blue: $a = 5,\ b = 2.$
$H = 7.$

In the red lines we see that the small waves and acute angles are in the majority; in the blue the big and low waves. Regarding direction, all the blue lines are horizontal like the lines of group II (quiet and its synonyms). In the red lines the different directions occur in equal numbers. The introspection of the subjects gives a key to the explanation of the lines.

G: The red line is the line of gaiety, of warfare. A cruel line.
L: Red line like the furious and the fiery.
S: Red line like fire.
D: Red line like anger.
F: Red line fiery.
Ma: Red implies great intensity, energy, and saturation.
G: The blue line is gentle, placid, and calm.
L: Blue line is serene like quiet water.
S: Association from the quieting blue sky.
D: Blue line is sad.
F: Blue line is calm and faint.

In some cases no introspection was given by the subjects. There is a certain interest in observing that with the color lines concrete associations seem to have been more lively

than with the other lines. If we connect the facts noted with our previous results it is quite obvious that the red line is the active and energetic type, while the blue is quiet.

The material gained from the drawing of beautiful and ugly lines can not be classified according to our previous basis of separation. The types of lines are too varied. The introspection, nevertheless, hints at general tendencies in drawing those lines.

BEAUTIFUL

G: Gradual curves. The line gives what is expected. No interruptions in it. A beautiful line is always symmetrical. There is a dignified quality in the beautiful line. It is smooth.

L: Graceful movement is connected with a beautiful line. It glides along and is always made up of curves. Association: waves of the sea.

Mi: A beautiful line has continuity and is not crossed by any other line. Association: a swan.

S: A circle is a beautiful line. It is satisfactory because it is symmetrical and curved.

C: A beautiful line swings smoothly and is round and curved. All the curves have the same form. Smoothness and roundness are necessary for a beautiful line.

D: A beautiful line has a harmonious fusion of its different parts. It has a certain symmetry and very little conflict. It expresses a single and harmoniously blended idea.

F: A beautiful line is symmetrical, smooth and round.

Ma: Gradual transition is necessary in a beautiful line. It cannot have any straight parts. It is sweeping.

UGLY

G: The ugly line is just like a mass. Curves, angles and straight parts are mixed together, without organization. You get ready for a curve and then you have something else. Constant changes from unpleasant to more unpleasant.

L: An ugly line must have angles.

Mi: An ugly line has no continuity. Straight lines, curves and angles are mixed up.

S: The ugly line goes nowhere. Has no feeling.

C: The ugly line is a sort of conglomeration; is meaningless and broken up. It is like a mass. No uniformity in the curving. The intersections make the line ugly.

D: Ugly lines have no unity, no harmonious fusion between curves, straight lines, and angles. A conflict of different emotions is caused thereby. The curved line has one feeling-tone, the angles another, and the straight parts a third. Even a conflict of movements.

F: The ugly line is asymmetrical and has angles.

Ma: In ugly lines there is a conflict between different parts. The unrelated spaces make the line ugly.

We can easily deduce from these introspective statements that certain qualities are necessary in a line in order that it will appeal to us, and that the absence of these qualities makes

the line ugly. The chief criteria of a beautiful line seem to be the following: unity in direction and movement, continuity, absence of angles and intersections, a periodical return of the same elements or a certain symmetry.

Summary

A résumé of the chief results regarding the affective tone of lines gained through the experiments can be formulated in the following sentences.

There seems to be a feeling tone connected with pure lines, which is perceived by a majority of observers.

This feeling tone is probably dependent upon the suggestion of movement in the lines; that is, the lines appear to imitate in their movement the motor expression of emotional states.

Slow and weak movement is suggested by lines with long and low waves, rapid and intense movement by lines with small waves and acute angles.

Consequently, emotions with little motor expression are suggested by lines of the former type and emotions with strong motor expression by lines of the latter.

A finer differentiation of the emotions suggested is not possible. Sad, lazy and quiet lines have practically the same shapes; likewise the furious and exciting are similar.

When sharp angles predominate in a line of the active type, it seems to acquire an unpleasant feeling tone, as well as an increased intensity in its movements. Thus, a jolly and a furious line differ in the frequency of the acute angles.

Strength is very often expressed by big and high waves; and even by right angles at times.

Broadness of a line indicates strength, while thinness indicates weakness and faintness.

Beauty in a pure line is expressed by unity of direction, continuity, roundness of curves, lack of angles, and periodical repetition of similar elements, or by a certain symmetry; ugliness by the reverse of all these qualities.

With these facts in mind we can understand why pure lines are an emotional factor in art and why they are an important factor in our enjoyment of the great masterpieces.

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SYSTOLIC BLOOD PRESSURE SYMPTOMS OF DECEPTION

BY WILLIAM M. MARSTON

1. INTRODUCTION.

The investigation on the galvanic effects of hidden ideas which have proved of interest both from a psychological and from a legal point of view have turned attention to the general problem of the physiological facts of the mental attitude in deceiving. The special problem suggested to me in the Harvard psychological laboratory was an investigation of the changes in blood pressure resulting from an effort to hide the truth. Just after we had begun the work, Benussi reported an experiment which in a parallel way studied the effect of lying on the changes in respiration.[1] He found a characteristic ratio of inspiration to expiration symptomatic of what he calls "internal excitement" caused by lying, and furthermore found this internal excitement to be much stronger in clever liars than in those easily detected, while, in the case of the latter, such excitement often tinged and modified the truthful records. Benussi, however, did not attempt to explain or analyze this "internal excitement," and his work leaves us with several troublesome questions in the answering of which this and similar methods of investigation might be invalidated. Is the "lying complex" sufficiently uniform in different individuals to be experimented upon as a unit? Through what physiological mechanism are symptomatic bodily changes effected? What, psychologically, is the nature of this "internal excitement"? Until these questions are at least partially answered we have in hand only a sort of psychological patent medicine, the ingredients of which, being unknown, may work as well one way as another under new conditions. Since, however, no definite emotional tests have

[1] *Archiv fur die Gesampte Psychologie*, 1914, pp. 244–271.

yet been established, a method similar to Benussi's seems inevitable in opening up a very complicated field. Benussi's results, indicating as they do great definiteness of lying symptoms, are sufficient to warrant the assumption of the uniformity of the deceptive consciousness as a working hypothesis. It will be the purpose of the present paper, in reporting the results of research on effects of this deceptive consciousness upon systolic b. p.[1] to analyze the data with a view of determining the physiological and psychological mechanisms involved.

2. Chief Physiological Factors of B. P. and Possible Psychological Influences upon these Factors

The blood, starting in the left auricle of the heart, is forced by the successive contractions of both auricles and ventricles into the aorta, or arterial stem. Thence the squeezing of the heart muscle forces the blood through the smaller arteries, and finally through the arterioles and capillaries into the veins, whence it returns under constantly diminishing pressure to the heart. In order to study psychic influences upon the b. p. it is necessary to bear clearly in mind the normal pressure conditions throughout the closed blood circuit. The pressure in the aorta is, of course, highest, and the broad channel of this artery offers comparatively little resistance to the blood flow; but as the smaller arteries are reached the factor of the friction with the arterial walls becomes more and more manifest, the side pressure and velocity pressure become less and less, and by the time the capillaries are reached the wall resistance is the dominating factor. The four chief factors in determining the arterial pressure at any given time may be said to be: (1) Heart-beat. It will be noted that the *rate* of the beat, and the *force* of the beat are two distinct functions; the former often increasing in inverse proportion to the b. p. while the pressure always increases in direct proportion to the force of the beat. (2) Constriction of the arteries, and especially of the arterioles and capillaries, usually called "peripheral resistance." (3)

[1] Abbreviation "b. p." will hereafter be used for "blood pressure."

Changes in elasticity of arterial walls. (4) Loss of blood. Since the last two factors are caused only by disease or by contingencies impossible of occurrence during the time occupied in taking any b. p. record, we need not consider them here.

Both the increase of heart beat and the increase of peripheral resistance, however, are factors to which we must look in accounting for any centrally caused changes. The systolic b. p. is peculiarly symptomatic of change in heart beat, while the diastolic becomes the crucial criterion if the changes we wish to study are brought about through changes in peripheral resistance.[1] Should both of these factors prove of essential value, then we must determine the pulse pressure, or ratio between systolic and diastolic. In order, then, to fix upon that aspect of arterial pressure which is the true indicator of that psychological complex which we wish to investigate, we must first glance briefly at the innervation of heart and capillaries, as well as at the psychological elements which have been found to substantially effect their functioning.

The rate and force of the heart beat are the algebraic sum of the cardio-inhibitory and accelerator nerve fibers. Thus a severing of the vagus nerves, or an inhibition at the cardio-inhibitory center will increase the heart beat as strongly as will a strong stimulation of the accelerator nerves. The latter seem to contain two groups of fibers capable of independent functioning, one group increasing the rate of beat, and the other increasing the force of the beat. It is only, however, through a reciprocal b. p. mechanism that the two groups function separately, central impulses seeming to set both groups in action. In the same way the peripheral resistance may be said to be the algebraic sum of the vaso-constrictor, and vaso-dilator nerves. Here, however, we must not think of peripheral resistance as a single organ or unit, but must remember, for instance, that the capillaries of the splanchnic area may be contracted while those of the skeletal muscles may be dilated. While, then, it is true that a strong vaso-constriction of an important area will immediately modify

[1] See *Boston Med. and Surgical Jour.*, Vol. CLXXII., No. 14, p. 530.

the diastolic b. p. of any artery above that area in the blood-circuit, it will only be a very general vaso-constriction which will have a significant effect upon the systolic pressure.

Since Professor Cannon recently definitely correlated six emotions with branches of the autonomic nervous system,[1] it will be well to further note the effects of these branches upon mechanisms regulating the heart beat and vaso-motor systems. The cranial division of the autonomic stimulates the cardio-inhibitory nerves, and the vaso-dilators of the stomach and digestive organs. The sacral division does not, in itself, innervate the heart, but only stimulates the vaso-dilators of the external genital organs and regulates the functioning of the excretory organs. When the excitement of the sacral system becomes sufficiently intense, the impulse passes over into the sympathetic. The thoracico-lumbar, or sympathetic, acts most uncompromisingly upon the accelerator nerves of the heart, and secondarily it inhibits the action of the digestive organs, contracting the blood vessels of these organs, and thus driving the blood to the skeletal muscles and outer parts of the body. The adrenalin released by the sympathetic impulses accentuates and prolongs this effect.

What emotions, then, will express themselves by heart acceleration, and which by vaso-constrictions? The answer is far from definite. The point first to be noted, however, is the uncertainty of any emotional influence through the vaso-motor apparatus upon b. p. The mild appetitive emotion is registered in the cranial division of the autonomic, and, consequently, has a vaso-dilator effect which, with the cardio-inhibitory action of this division, would be expected to diminish b. p. Yet, through a peculiar inhibition of the cardio-inhibitory center, a slight increase of pressure actually occurs. In the same way sex-emotion and relief, expressing themselves through the sacral division, would seem to tend to lower diastolic pressure through vaso-dilations, yet early in the development of sex excitement the sympathetic is aroused, and, until the climax is nearly reached, the effect

[1] W. B. Cannon, "Interrelations of Emotions," *Am. Jour. Psy.*, Vol. XXV., pp. 256–282.

upon systolic b. p. is scarcely discernible. Again, pain, according to Cannon, is one of the three major emotions normally expressing itself through the sympathetic division, and should, therefore, both increase the heart-beat and contract the blood-vessels in large visceral areas. There is no reason to doubt that such vaso-contractions occur, yet Binet early reported,[1] and his report is confirmed by the vivisectionists, that only the diastolic pressure is significantly altered by pain, that the heart is slowed in rate, and if there is any increase in systolic pressure it apparently is produced by the compensatory b. p. mechanism which operates to increase the force of the beat when the rate is diminished. Thus far, then, we have seen that the expression of emotion in vaso-motor modifications has little or no significance in determining the b. p. which would seem to be much more strongly and significantly controlled by the heart under normal conditions.[2] The one striking exception to this general rule forms the final argument for the choice of the systolic in testing the deceptive consciousness. Binet found that intellectual work at high concentration increased the diastolic b. p. 20, 30, and even 40 mm. The explanation is clear when considered in a teleological light. All the blood is required by the brain, and consequently, through vaso-constrictions, it is driven away from almost all other parts of the body. Yet, as I will indicate a little later, the systolic b. p. is not increased, nor is it significantly modified by even 40 or 50 minutes of mental concentration on study.[3]

The foregoing summary of the effects of minor affective elements and of intellectual work on systolic and diastolic b. p. will, I believe, justify the choice of the systolic in testing the deceptive consciousness. First, the use of the systolic eliminates the local effects of minor affective states; secondly, it eliminates the important and irrelevant factor of intellectual

[1] Binet et Vaschide, "Influence du travail intellectuel des Emotions, et du travail physique sur la pression du sang," *L'Anee Psy.*, 3, pp. 129–183.

[2] In medical diagnoses, of course, the peripheral resistance is often, on the other hand, the very condition to be investigated.

[3] Here we find the compensatory heart mechanism decreasing the force of the beat with the increase of rate.

work; thirdly, it is less susceptible to modification by physical pain than is the diastolic; and fourthly, it tends to record only the unequivocal changes in the b. p. system brought about through increase of heart-beat unimpeded by inhibitory reflexes or antagonistic functioning of the vaso-motor apparatus.

What mental processes may be expected, then, to cause an increase of heart-beat, and consequent rise of systolic pressure? We have, first, Cannon's three major emotions, rage, fear, and pain expressing themselves in the sympathetic division of the autonomic. Pain, as we have seen, has a less marked effect upon b. p. than the unobstructed operation of the accelerator nerves would seem to argue; yet it is possible that Professor Cannon had in mind not only physical pain, but also "psychic" or mental pain, which would resolve itself into an extreme degree of unpleasantness derivable from many sources. However, we may point to fear and rage as emotions which will, through the unobstructed operation of the sympathetic division, cause immediate rise of systolic pressure. It is well to note that only the smallest degree of fear or rage should, theoretically at least, be necessary to produce a rise in b. p. since the sympathetic system is the natural avenue for the expression of these emotions. Cannon also finds that sex-excitement, intense joy, intense sorrow, and intense disgust may, when they reach a sufficient level of intensity, break over into the sympathetic channels,[1] where they are felt merely as "excitement." Thus it appears that a profound modification of systolic b. p. cannot be analyzed with respect to its ultimate psychological causes, while any persistent smaller rise may presumably be attributed to rage or fear. Of course, very slight increases of pressure (especially if this be recorded by comparatively crude methods) cannot be regarded as significant of anything, but the necessary degree of intensity for emotions other than rage or fear to break into the sympathetic would seem to be so high that a considerable range of significant modification can be regarded as attributable almost exclusively to fear and

[1] W. B. Cannon, Ibid., p. 270.

rage. Although it is impossible to fix definite boundaries for this field, I shall hope to point out its general demarkations in considering experimental results. It only remains to point out, as a preliminary caution, the strong effect of physical exertion and of any contractions of skeletal muscles upon the accelerator nerves, and consequently upon the systolic b. p. All records must be keenly scanned, and conditions carefully controlled with a view to the elimination of the influence of this factor from the results. It is, however, much easier to control and allow for the factor of physical exertion, than it would be to exclude the element of mental work were we to use the diastolic or pulse pressures in examining the deceptive consciousness.

3. Method

The b. p. measurements were taken with a "Tycos" sphygmomanometer, an instrument substituting a spring for the mercury column of the older apparatus, and having the rubber pressure bag contained in a silk envelope made to wrap conveniently around the arm of the subject. The pressure was taken in the left brachial artery, the arm being completely bared before adjusting the instrument. This method of measuring the systolic pressure depends, of course, upon detecting the pulse in the radial artery, either by sphygmograph or by tactile sensations of the experimenter. Since the latter method was employed, the experiment is open to the criticism that the pulse is often present for a time after it has become impossible to detect it by mere touch. However, it may fairly be said that mechanical detectors have scarcely greater sensitivity, and are, in the long run, vastly less reliable. Moreover, the correction of this crudity of method would rather tend to accentuate increases in pressure than to diminish those found. Before starting the experiment, the experimenter practiced the taking of b. p. daily, for several weeks.

Four series were run off. The first three will be treated together, having been used upon the same group of ten subjects; and the last series, which employed the same method, will be introduced later, merely as a checking series.

Series A. (*Stories 1–8.*)—The subject came to the experiment as to an examination by a prosecuting attorney, resolved to save a friend who was accused of a crime. He sat down at a table beside the experimenter (but protected by a screen) and found on the table two papers face down; one marked "L" (Lie) and the other marked "T" (Truth). If, in saving his friend, the subject chose to lie, he turned over and read the "L" paper. This was a story prepared by the experimenter relating simple events, supposed to have been witnessed by the subject, and proving the friend guilty. At the end of the story were recorded certain facts, supposed to have been established by other witnesses, which the subject must admit in forging an alibi for his friend. He then proceeded, with these facts and the true story before him, to think out a consistent lying alibi. If the subject chose to tell the truth, he turned over the "T" paper, the contents of which were unknown to the experimenter, and found a consistent story, admitting the facts supposed to have been established, but completely exonerating his friend. This story was the *truth*, it was the only account he knew of the affair, and he told it as such. In either case the subject had 10 min., or until he announced he was ready, to thoroughly familiarize himself with the story he was about to tell, but was free to refer to the chosen paper any time he wished. The experimenter had prepared ten questions covering the incidents of the "L" story and an assistant had prepared the "T" story to successfully cover the questions, and the facts supposed to have been established. (After one "T" story has been told, of course, another was prepared by one of the assistants.) Thus it was impossible for the experimenter and jury to know whether the subject was telling a story of his own, or the one composed by the assistant. The questions were then put to the subject, and the jury closely observed his manner while answering. They then rendered a "verdict" as to whether he had lied or not, basing their judgment upon the internal consistency of the story as well as upon the subject's appearance while answering questions. These verdicts were written and passed in. The jury then left the

room, and the experimenter recorded his own judgment, which was based entirely upon the b. p. record. The subject gave his introspection, a final reading was taken and the instrument removed. The b. p. was recorded five times, in each experiment of Series *A*, (1) before the subject turned over the paper, (2) after he announced that he was ready, (3) after the fifth question, (4) after the last question, and (5) after his introspection. Complete notes of the subject's story were taken by the experimenter.

Series B. (*Stories 9–10.*)—The method of preparing the stories, etc., was exactly the same as in Series *A*. In Series *B*, however, the subject was first allowed to tell his own story without any questioning or interruption, and was then cross-examined by the experimenter or by the jury, or by both. If he chose he was permitted to reply to any question that "he did not know," or that "it wasn't on the paper." The jury then rendered their verdict orally, after whispered discussion, and the experimenter delivered the b. p. verdict orally. The jury then filed out, and the subject gave introspection, or in some cases, remained quiet for some moments. The essential innovation in this series was taking pressure readings every 2 min.

Series C. (*Experiments 11–12.*)—In this series the subject received an envelope with instructions sealed therein. He immediately left the room, and if he chose to lie he opened the envelope, obeyed the instructions, and came back and lied about what he had done. If he chose to tell the truth, he did whatever he liked for 10 min. and came back and gave an accurate and truthful account of his actions. After his return, he was given 2 to 6 min. to get his account in mind. In No. 11 no record of b. p. was taken until the subject returned, but in No. 12 a record was taken before he left the room. Owing to the lateness of the college year several subjects left before this last series could be completed.

Since Series *A*, *B*, and *C* can conveniently be considered together, I have tabulated together the results of these series for each subject.

The experiment was performed in the Harvard psycho-

logical laboratory, during the academic year 1914–15. Six subjects were graduate students of psychology, and four were undergraduates of considerable psychological training. The jury varied in number from 2 to 10, and was made up of men from Professor Münsterberg's elementary course. Beside this regular panel, several research students not in the experiment sat occasionally on the jury and numerous other students of psychology frequently acted as jurors. All who took part were greatly interested in the experiment, and all the subjects took the task of deceiving the jury very seriously, doing their utmost to outwit both jury and experimenter. The subjects were instructed at the beginning to choose an equal number of "T" and "L" stories, but although a list of previous choices was kept for each subject, this instruction could not be repeated without marring the conditions of the experiment. As a result, the subjects usually chose to lie more frequently than to tell the truth.

4. Results

A. Intellectual Work

In the above summary of physiological factors involved in systolic b. p. it will be remembered that the statement was there made that no one of such factors was influenced by intellectual work. This statement is substantiated by preliminary tests made upon all the subjects who took part in the experiment. B. p. records were taken while the subjects were doing arithmetical work at high concentration, while they were studying for college courses, and several extra short records were taken while inventing stories similar to the ones necessary for this experiment. A few significant variations were found, all of which could be directly correlated with some intense emotional intrusion; but during the actual mental concentration no uniform curve could be found either for all the men, or even for different records of one subject. No rise of more than 4 mm. was noted, and although the general tendency seemed to be a diminishing of pressure during a long period of mental work, no very significant or uniform descent of the curves could be noted. A single typical curve

will present the general results on this point as clearly as would an extended tabulation.

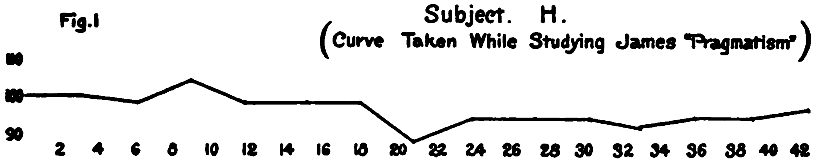

FIG. I

It will be noted that this record extended over 42 min., in which time the subject covered 34 pages, with an excellent mastery of the thought therein contained, pressure being taken every three minutes. The major part of the curve is well below the initial pressure, and the single rise of 4 mm. is neither sufficiently high nor sufficiently prolonged to be significant. How then shall we account for such small, irregular fluctuations? It seems that we need not look beyond purely physiological causes. Besides the frequent minor irregularities of normal heart beat and vaso-motor adjustment, the factors of respiratory waves of b. p. and the longer waves due to rhythmical variations in the tonicity of the vaso-constrictor center under unusual conditions must be taken into account. In the light of the numerous and inevitable variations due to such constantly acting causes, it seems a safe general rule to regard no systolic variation below 6 mm., and perhaps none below 8 mm., as significant of major emotional influence. It is certainly possible that the intellectual work, besides raising the peripheral resistance and diastolic pressure, may also effect respiratory and minor chemical changes which cause systolic variations; but such physiological effects, at all events, seem to depend largely upon the temporary condition of the individual organism, and so may be dismissed as unimportant for the purposes of the present experiment. A careful study of the pressure changes during the "preparation" periods of the experiment shows a general result very similar to the preliminary intellectual work tests; while correlation of introspective reports of where intellectual work became necessary in the course of

the narrative with the pressure record at such points fails to show any increases of pressure.

B. General Results of Effect of Deceptive Consciousness

In a word, a uniform and significant systolic pressure curve was established by the results, as symptomatic of the deceptive consciousness. A rather surprising secondary result was the appearance of an almost equally definite Truth curve. Before proceeding to analyze the general aspects of the significance of these b. p. modifications, it may be well to note the two most general and plausible doubts which can be advanced concerning the possibilities of significant results under the conditions of this experiment. (1) Is the necessity for deceit sufficiently vital to furnish emotional stimuli for significant rises in b. p.? (2) If a sufficiently intense emotional situation is produced, will not the presence of witnesses, etc., cause exactly similar emotional influence while the subject is telling the truth? Let us glance, in answer, at the highest lying curve obtained in contrast with the lowest truth curve of the same subject, and at the lowest lying curve obtained contrasted with the highest truth curve of that subject.[1]

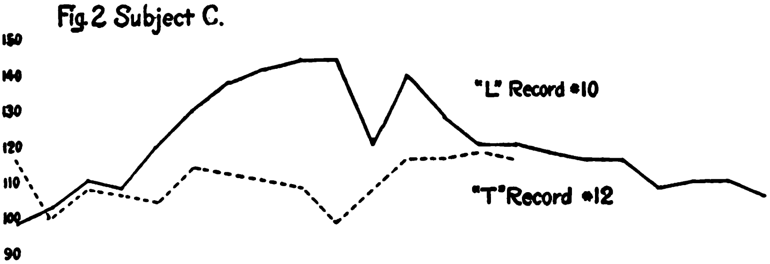

FIG. 2

The two *most* extreme curves, taken from the record of Subject *C*, conclusively indicate that neither doubt invalidated all the records taken, and the curves representing *least* extreme b. p. differences for truth and lying establish the significance of the pressure modifications in all the tests taken. In No. 10, Subject *C*, it can not be doubted that some very

[1] It would be obviously impossible to compare the lying curve of one subject with the truth curve of another, on account of the individual differences of initial and average pressure.

strong emotional situation was produced by the conditions of the experiment, since it will be noted that a total rise of 46 mm. in the b. p. was recorded. In No. 12, on the other hand, the central processes of the same subject, under the same conditions of witnesses, cross-examination, etc., caused a *drop* in the b. p. of 18 mm. toward the end of the cross-examination, with a return to initial pressure when the story was over, and a rise of only 2 mm. as the verdict was delivered. This and similar records seem to indicate very clearly that, during the telling of a truthful story to a suspicious and critical audience there is a more or less typical emotional (or other central) grouping of conscious factors which tend to inhibit any general emotional reactions to environment capable of increasing pressure, and which exert a positive influence over physiological conditions. How strong and consistent this influence may be can only be determined by a study of the individual results. The 8 mm. rise shown in record No. 11,

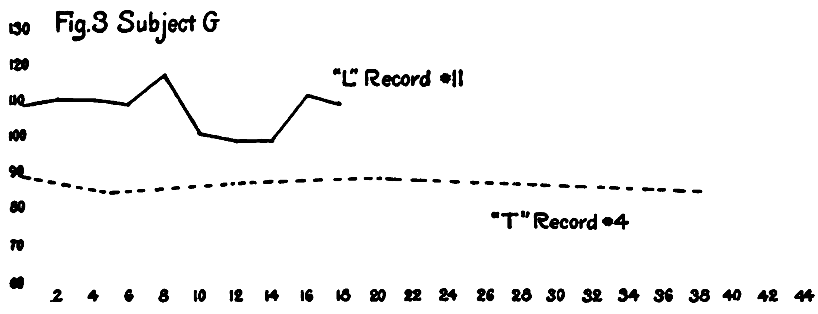

FIG. 3

Subject *G*, is the smallest b. p. increase recorded during deception by any subject and appears just on the edge of the field of pressure modifications significant of major emotional influence. It would seem that deception wrought little havoc in Subject *G*'s emotions, but when we glance at record No. 4, we see that while telling the truth, *G*'s b. p. did not rise *at all*, dropping 4 mm. in his highest truth record. The results, then, of this experiment unquestionably show significant b. p. changes under the influence of the deceptive consciousness.

But what constitutes the "significance" of a pressure curve symptomatic of deception? What differentiates such a curve from any chance rise of b. p. caused by the arousal of some incidental emotional complex? The answer to these questions is to be derived from a study of the above typical curves.

1. The amount of the rise is, in all "L" curves, too great to be accounted for by moderate degrees of intensity of any emotions other than fear or rage, the minimum rise being 8 mm.

2. The duration of the rise is, in "L" curves, too long to be symptomatic of a sudden and transient emotional association, the minimum duration of any rise being 8 minutes.

3. The rise of an "L" curve occurs in regular, climactic manner. The pressure starts its rise close to the beginning of the recital in every record as in the typical curves above, climbs with varying abruptness but with great consistency of movement to a definite climax, and then recedes. Subsequent questions may cause secondary climaxes, but these are patently subsidiary to the steady, persistent climb and fall of the pressure curve taken as a whole.

4. The apex of each curve is correlated very closely with that point in the subject's testimony which marks the crisis, or climax, of the whole "job" before the subject. This was determined partly by introspection, but chiefly by observations on the manner and attitude of the subject, and by noting the whole construction and plan of the false "alibi." Thus, like the other elements of "significance" in "L" curves, such correlation is capable of *objective* determination.

FIG. 4[1]

Subject	*A*	*B*	*C*	*D*	*E*	*F*	*G*	*H*	*I*	*J*
Av. rise in "L" curve	12.6	14	26.7	18.5	17.3	17.2	14	15	10.8	18.2
M. v.	2.2	4	10.2	7.7	3.3	4.9	4	1	1.7	3.4
Highest rise in "T" curve	+6 and −10	+8	+18 and −4	+4 and −4	+6	+14	−4	−6	−10	−6

The table in Fig. 4 will present a rough summary of the

[1] Since closer fractional determination would be meaningless, the averages are given as of the nearest millimeter.

general results, the extremes of which have already been shown in Figs. 2 and 3.

It will be noted, from this table, that every subject's average b. p. increase during deception is well above his highest "T" curve, and that, with 2 exceptions, the highest "T" pressure mark plus the m. v. from the "L" increase is still below the average "L" rise. It is further true, although it does not appear in the above table, that, with the same two exceptions, every subject's lowest "L" curve was significantly higher than his highest "T" curve. The m. v. s. are, on the whole, low, seeming to suggest a rather fixed amount of b. p. increase for each subject, although the number of measurements is by no means sufficient to prove such a generalization.

A total average of 16 mm. rise in b. p. during 56 deceptions, by 10 different men, all such "L" curves having the significant characteristics pointed out above, seems conclusive proof of marked modification of b. p. during deception.

C. *Individual Results*

In order to determine, if possible, the psychological causes of the b. p. modifications during deception, as well as to study the uniformity of a possible truthful complex, it is advisable to review the individual records, subject by subject. Only in this way can the curiously close correlations of introspection and pressure record, the individual peculiarities, and certain interesting mixtures of truth and lying be considered. I shall attempt to bring out these salient features very briefly with each set of records, summarizing, thereafter, my own conclusions as to meaning and interpretation.

Subject A.—The stories composed by Subject *A* were, on the whole, very poor alibis. They were rambling, indefinite, rather wild, and very improbable, yet while telling the truth this subject managed to convey, by his peculiar manner of narration, the impression that he was lying, so that the jury found it very hard to judge correctly in any case. Subject *A* introspected during deception, a feeling of "responsibility" and *fear* of questions to come. He found lying "restful,"

FIG. 5[1]

Subject A

T					
2		4		11	
	120		116	2 2	118 118 118
End of preparation			←		←
	118	6	114	2 2 2	118 124 108
End of ques. 1–5			←	End of recital	
	120	11	118	2 2 2 2 2 2	118 116 114 116 108 108
End of ques. 6–10			←	End of cross-exam.	
	118	9	116	2	112
End of introspection			←	Verdict given	
10	120	10	116	2 2	114 118

[1] Number at head of each vertical column indicates which story was used in that record. Three digit numbers are mm. b. p.; and one or two digit numbers in narrow columns indicate min. s. elapsing between b. p. readings.

FIG. 5 (*Continued*)

Subject A

	L										Conscious of Detection		T and L	
1	3		6		7		9		10		5		8	
120		118		120		118	3 2 2	116 120 124 122	2 2	118 120 124		118		128
←		←		←		←		←		←		←		←
124		128	6	124	4	130	2 2 2	128 128 126	2 2 2 2 2 2 2	130 134 136 130 130 126 126	6	112	5	128
f ques.		←		←		←	End of recital			←	End of ques. 1–5			←
128		124	10	132	7	128	2 2 2 2 2	126 122 124 126 126	2 2 2	124 128 122	19	112	11	124
f ques. ɔ		←		←		←	End of cross-exam.			←	End of ques. 6–10			←
100		132	8	116	11	126	2 2	118 116	2	120	8	118	10 4	122 124[1]
f intro- tion		←		←		←	Verdict given			←	End of questions by jury			←
118		114	12	122	18	120	2 2	116 118	2 2	128 122	10	108	2	128
							End of intro-spection			←	Period of quiet			←
							2 2	116 116	5	118	10	108	11	126

Lied

lax, and pleasant; but while telling the truth his feeling tone was "indifferent."

Notable Individual Records. No. 3.—At the 5th question subject felt "relief," and "elation" at supposed success in fooling jury. It will be noted that the pressure falls at this point, and rises during introspection when "*worry*" was felt lest he had not told a good lie after all.

No. 5.—Objectively, this story was a wild lie. Yet at the very first question, subject realized that he had betrayed himself to the jury and experimenter, and felt "disgust," "shame," and subsequent boredom. It will be noted that, unlike most of *A*'s "T" records, this consciousness produced a consistent drop in pressure. The most salient characteristic of the introspection was utter lack of interest and complete relaxation. No. 6 shows a similar drop from 132 to 116 in 8 min. after subject betrayed himself, consciously, in the 10th question.

No. 8.—Subject ran hard, just previous to coming into experiment, for about a quarter of a mile. The persistence of the influence of strenuous physical exertion is to be seen in the record, 26 min. being required for pressure to return to 122 (approximately normal). A single lie, told in answer to the last question, with introspective confession of this lie, sent the pressure up again 6 mm. in as many minutes.

Subject B.—*B* introspected, when lying, *fear* of many things, and it was for this reason that he did not choose to lie more often—he feared to fear! although, when it was over, he found he enjoyed the deception more than the truth. His stories were both good, although not ample, and his manner of telling both truth and falsehood was even and quiet. *B* felt "tense" during both deception and truth, the lying itself being more pleasant, and keeping him more "alert." It will be noted that a majority of *B*'s "T" records show consistent downward tendency, with a return toward the initial level.

Notable Individual Records. No. 8.—It will be noted that a severe pain raised the whole level of the day's blood pressure much above the subject's average level, and that this influence

evidently counteracted the usual downward tendency of *B*'s "T" curves.

No. 9.—The pressure was probably sent up, during the recital, by odd facial expressions involving considerable

FIG. 6

Subject B

T										L			
4		6		8[1]		9		11		5		7	
	96		90		106	3 2 2 2 2	84 86 86 88 88 86	2 2	102 106 94		88		*120*
End of preparation			←		←		←		←		←		←
21	84	10	84	3	106	2 2 2 2	86 90 84 92	2 2	88 88	9	96	4	*138*
End of ques. 1–5			←		←	End of recital			←	End of ques. 1–5			←
6	88	6	92	9	108	2	86	2 2	92 94	9	98	7	128
End of ques. 6–10			←		←	End of cross-exam.			←	End of ques. 6–10			←
8	88	6	92	8	104	2	86	2	96	7	92	14	112
End of introspection			←		←	Verdict given			←	End of introspection			←
8	92	10	96	10	108	2 2 2 2	88 100[2] 88 90	2 2 2	98 94 98	11	90	10	110
						End of introspection							
						2 2 2 2	90 90 88 88						

[1] B has *severe toothache.*

[2] Muscular contractions.

muscular contractions. A contraction of the left bicep, just as the pressure was taken, accounts for the abrupt rise to 100 mm. after the verdict was given.

No. 11.—A little physical exercise probably raised the initial pressure 2 to 4 mm.

No. 7.—Introspection revealed that for some minutes before coming into the experiment, *B* had been planning to deceive the jury. This would seem to have raised the initial pressure well above normal level (*B* had done no physical exercise), the actual lying sent it still higher, and it was still on the downward trend toward normal level when *B* left.

Subject C.—This subject did a great deal of laughing, but aside from this made an excellent witness, telling very plausible complete lies. It will be noted that, nevertheless, *C*'s blood pressure modifications were greater, during deception, than those of any other subject. He found lying "easy," but, while deceiving, he felt "like during an exam," "nervous," and "embarrassed." Nevertheless he felt more "tense" during truthful stories, and found them unpleasant, since he felt he could not make the b. p. rise. The apparent great elasticity of this subject's b. p. is to be noted.

Notable Individual Records. No. 2.—*C* introspects that he "worked hard" to raise the b. p. by suppressed laughter, and this is literally true. This "suppressed laughter" involved strenuous muscular contractions all over his body. Such contractions, as would be expected, sent the b. p. up, but it is to be noted that the rise did not follow the form of a lying curve. The b. p. was simply raised 8 mm. and kept there as long as that kind and amount of muscular contraction continued.

No. 7.—More laughter evidently sent the pressure up slightly; but it is probable that its initial level was not the day's norm, and that such laughter had little influence beyond initiating the return to such average level.

C made a mistake in questions 8 and 9, owing to a slip of memory, and recognized that he had made it, but did not correct it lest he be thought to have lied. It is very significant to note that such uncorrected mistake caused no rise of b. p.

FIG. 7

Subject C

T							
2		4		7		12	
	100		110		94		118
End of preparation			←		←	C returns	
	118	10	99	7	102	10 2 2	102 110 108
End of ques. 1–5			←		←	End of preparation	
	118	11	112[1]	10	104	2	106
End of ques. 6–10			←		←	End of recital	
	118	7	110	17	104	2 2 2 2 2 2	116 114 112 110 100 110
End of introspection			←		←	End of cross-exam.	
	96	18	106	15	104	2 2	118 118
						Verdict given	
						2 2	120 118

[1] Suppressed laughter.

FIG. 7—*Continued*

Subject C

L

1		3		5		6		8		9		10		11	
	112		104		118		116		112	2 2 2 2	118 120 122 124 126	2 2 2 2	100 104 112 110 122	2 2	130 126 102
End of preparation			←		←		←		←		←		←		←
	120		126	9	122	8	126	8	130	2 2 2 2 2 2 2 2 2 2	130 140 146 136 142 136 146 146 140 136	2 2 2 2 2 2	132 140 144 146 146 122	2 2 2	130 138 142
End of ques. 1–5			←		←		←		←	End of recital			←		←
	128		128	10	130	16	118[1]	9	130	2 2	120 134	2 2	142 130	2 2 2	146 140 144
End of ques. 6–10			←		←		←		←	End of cross-exam.			←		←
	128		134	7	124	6	122	12	126	2	138	2	122	2 2 2	130 126 122
End of introspection			←		←		←		←	Verdict given			←		←
	118		102	19	108	14	108	5	118	2 2	122 120	2 2 2 2 2 2 2 2	122 120 118 118 110 112 112 108	2 2	130 132
								Period of quiet.		End of introspection.			←		←
								16	108	2 2	122 116	2	104	2	120

No. 10.—This was highest curve obtained for any subject, and *C* introspected marked *fear*, throughout, which he attributed to the fact that he had not had time to thoroughly construct an alibi.

FIG. 8

Subject D

T					
8		9		12	
	102	2 2 2	92 92 96 94		102
End of preparation			←	D returns	
5	100	2 2 2 2 2 2 2	94 94 90 88 92 94 96	10 2 2	102 90 86
End of ques. 1–5		End of recital.		End of preparation	
7	100	2	90	2	90
End of ques. 6–10		End of cross-exam.		End of recital	
6	90	2	90	2 2 2	94 96 98
End of introspection		Verdict given		End of cross-exam.	
	90	2 2 2 2	96 94 94 94	2	90
		End of introspection		Verdict given	
		2 2	90 92	2	94

Fig. 8 (*continued*)
Subject D.

	L													T and L	
	1	2	3		5		6		7		10		11		4
	96	90	84		90		86		90	2 2 2	102 100 88 102	2 2	110 108 100		112
End of preparation		←	←		←		←		←		←		←		←
	102	102	84	7	90	7	102	10	108	2 2 2 2 2	110 128 116 106 96	2	126	6	110
End of ques. 1–5		←	←		←		←		←	End of recital			←	End of ques. 1–5	
	108	104	90	9	100	6	90	7	106	2	96	2 2 2 2	142 106 104 100	8	108
End of ques. 6–10		←	←		←		←		←	End of cross-exam.			←	End of ques. 6–10	
	90	98	94	4	90	8	92	10	106	2	98	2	96	8	122[1]
End of introspection		←	←		←		←		←	Verdict given.			←	End of introspection.	
	100	98	82	18	88	8	82	13	104	2 2	100 96	2 2	100 100	10	98
												End of introspection			
												2 2	96 96		

[1] Lied in answering Ques. 8, 9.

No. 11.—Short but strenuous physical exercise raised the initial b. p. level, but in 4 min. it had returned to 102 or about normal, from which point the "L" curve starts its steady climb.

Subject D.—The stories of this subject were largely negative and as scant as possible. *D* found deception pleasant, and lax, but introspected "*fear* as before an exam," with accompanying "contractions of the diaphragm." He also felt *angry* if forced with questions.

Notable Individual Records. No. 4.—In this record, *D* chose the "T" story, but feared from the first that it would not cover all the questions asked. This feeling seems to have led the subject to lie during questions 8 and 9, and it was only at this point that he felt "diaphragm contraction." It will be noted that at this point the b. p. rose 14 mm. in 8 min., and the experimenter was able to enter a correct judgment, based on this increase, as to the truth and deception of *D*'s story.

No. 5.—*D* tried to "beat the b. p.," by taking no interest in the deception, but it will be noted that the b. p. rose as usual. Also *D* introspected *fear*, and an "alertness despite himself."

No. 12.—Slight physical exertion before *D* came to the experiment at all may have raised the initial b. p. level somewhat.

Subject E.—*E*'s stories were racy, dramatic, but inaccurate and careless. He was very suspicious of all questions and directions, and had a great desire to outwit the experimenter. While lying he introspected "nervous excitement," inhibitions of ideas due to feeling "like stage *fright*" and "worry" as to b. p., although he found deception very pleasant, and telling the truth uninteresting and indifferent.

Notable Individual Records. No. 4.—*E* added several details to the "T" story, but claimed to regard this just as "telling it in his own words," and introspected "no excitement." From his manner and story, however, he seemed to have a lurking background of vaguely conscious fear that he would be caught up on some of these details, and it will be noted that the b. p. rose very evenly to the slight extent of 6 mm., rather in contrast to its usual more erratic behavior during "T" records.

No. 11.—Initial height of b. p. is due to short, strenuous physical exercise.

No. 12.—Subject while outside after the first b. p. reading sent b. p. up by smoking furiously. He hoped thus to keep it at a high level, and fool the experimenter, but it will be noted that it fell to normal level in 2 min.

FIG. 9

Subject E

T									
1		4		5		7		12	
	108		102		104		108		102
End of preparation			←		←		←		E returns
	98	8	104	4	98	4	108	10 2 2	126 106 92
End of ques. 1–5			←		←		←	End of preparation	
	112	6	108	19	100	9	110	2	106
End of ques. 6–10			←		←		←		End of recital
	96	15	108	7	92	15	108	2 2 2 2	104 102 94 104
End of introspection			←		←		←	End of cross-exam.	
10	100	12	106	7	96	12	108		102
								Verdict delivered	
									94

FIG. 9 *(Continued)*

Subject E

L											
2		3		6		8		10		11	
	99		100		94		94	2 2	90 94 114	2 2	128 114 114
End of preparation			←		←		←		←		←
	100		110	3	98	5	100	2 2 2 2 2 2 2	102 100 98 100 106 92 100	2	114
End of ques. 1–5			←		←		←	End of recital			←
	104		*112*	10	114	6	104	2 2 2 2 2	110[1] 110[1] 102 112 92	2 2 2 2 2	110 132 100 100 126[2]
End of ques. 6–10			←		←		←	End of cross-exam.			←
	106		106	8	102	12	110	2 2	112 104	2	100
End of introspection			←		←		←		Verdict given		←
	100		106	19	94	19	94	2 2	102 88	2 2 2	112 100 112
										End of introspection	
										4	106

[1] Confessed lying. [2] Jury asked betraying question.

Subject F.—Less weight is to be given to this subject's records than to those of any other subject, because of the many elements which would have to be carefully analyzed out.

He told wild, unconvincing stories, and his involuntary movements and stuttering should all have been recorded by a very complicated apparatus if his b. p. records were to be

FIG. 10

Subject F

T						L									
6		8		9		4		5		7		10		11	
	118		114	2 2 2	90 92 94 100		78		96		120	2 2 2	106 108 112 114	2 2	
End of preparation			←		←	End of preparation			←		←		←		
3	110	4	108	2 2 2 2 2 2 2 2	102 102 104 102 102 102 102 102	7	104	4	102	4	138	2 2 2 2 2 2 2	118 126 116 118 112 112 106	2 2 2 2 2	
End of ques. 1–5			←	End of recital		End of ques. 1–5			←		←	End of recital			
8	102	12	106	2	100	7	94	9	106	7	128	2	108	2	
End of ques. 6–10			←	End of cross-exam.		End of ques. 6–10			←		←	End of cross-exam.			
8	108	12	112	2	100	7	84	12	90	14	112	2	102	2	
End of introspection			←	Verdict given		End of introspection			←		←	Verdict given			
10	104	2	92	2 2 2	96 92 94	14	82	12	100	10	110	2	96	2 4	
		Period of quiet		End of introspection										End intros[pection]	
		10	114	2 2 2	102 96 90									6	

relied upon as a conclusive test of cerebral factors. During deception he felt "sneaky," cold, "carefree," pleasant, excited and "*frightened.*" During "T" stories he felt the "strain"

in trying to be accurate. He inhibited symptoms of extreme nervous *fear* during deception, such as chattering of teeth and trembling of hands.

FIG. 11

Subject G

T			L											
2	4		1		3		6		7		9		11	
104		90		102		82		98		104	2	92 94	2 2	118 110 112
prepara-		←		←		←		←		←		←		←
98	5	86		110		96	3	100	3	108	2 2 2 2 2 2 2 2	112 104 100 104 104 112 106 100	2 2	112 110
ques. 1–5		←		←		←		←		←	End of recital			←
84	7	88		100		78	9	118	9	114	2 2 2	104 100 102	2 2 2	118 102 100
ques.		←		←		←		←		←	End of cross-exam.			←
94	8	90		98		90	7	96	6	118	2	100	2	100
intro- ion		←		←		←		←		←	Verdict given			←
96	18	86		108		90	13	90	20	106	2 2 2	102 90 90.	2 2	112 110
											End of intro-spection			
											2 2	84 90	2	84

Notable Individual Records. No. 4.—Low general level of b. p. due, apparently, to lack of sleep.

No. 6.—Although the record shows a drop of 16 mm., subject was conscious of having made uncorrected mistakes.

Evidently whatever emotion accompanies this idea does not increase the b. p.

FIG. II (*continued*)

Subject G

T and L;			
5		8	
	88		102
	←		←
3	88	9	96
End of ques. 1–5			←
10	100	9	102
End of ques. 6–10			←
10	100	10	100
End of introspection		Ques. s. by jury	
20	102	11	112[1]
			←
		9	92

[1] Lied to jury.

No. 9.—Here the b. p. rose to 100 or 102, and was kept there pretty consistently until the subject had entirely finished speaking, not following the form of a curve of deception, but merely exhibiting irregular increases and slight

drops. This peculiar b. p. behavior is to be explained by intermittent body-shaking laughter, and bad stuttering throughout the narration and introspection.

Subject G.—Stories were very plausible, but not very complete and careful. During deception, *G* experienced slight inhibitions, and felt "worried" and "anxious." Telling the truth was harder and less pleasant for *G* than lying.

Notable Individual Records. No. 2.—It will be seen that this shows a typical truth record, and although outside the prescribed conditions of the experiment, the story told was strictly true. After reading the "T" story, *G* thought it improbable. Therefore, to correct an improbable tale he narrated incidents which actually had happened to him, and which, localized at the time and place of the alleged crime, formed a simple and complete alibi. *G* felt throughout that he was correcting a mistake, and telling the real truth, so that this record seems fairly listed as a real truth record.

No. 5.—*G* read the "*T*" story, intending to tell the truth. The second reading shows such was his intent at the moment he finished his preparation. But, yielding to an impulse to improve upon the story given, *G* began to enlarge upon it, with a consequent consciousness of deception and a rise of b. p. At the beginning of the introspection he continued to lie, telling me that he thought he was to use the story given merely as a synopsis but soon laughed and confessed his deception. It will be noted that the b. p. did not fall at the end. In all probability it was much higher at that point toward the end of the introspection where he reached the climax of attempted deception of the experimenter, and had started down toward normal level when the last reading was taken.

No. 8.—This is an excellent example of a simple "T" curve broken by a single lie.

Subject H.—*H* took less actual interest in any sort of work than any other subject and for this reason both the number and quality of his "T" records are significant. His lack of active interest, he introspected, led him to choose the truth 7 times out of 10, and the feeling persisted throughout these "T" records. It will be noted that every one is a downward

curve, varying in drop from 6 mm. to 16 mm. *H* found deception pleasant, but "disquieting," and "irritating."

Notable Individual Records. No. 3.—*H* tried to believe what he said to keep the b. p. down, but his failure is evident in the regular "L" rise of 16 mm.

FIG. 12

Subject H

T													
2		4		5		6		7		8		11	
	106		96		98		96		106		98	2 2	98 98 98
End of preparation			←		←		←		←		←		←
	104	4	94	3	98	4	92	4	90	6	92	2	88
End of ques. 1–5			←		←		←		←		←	End of recital	
	94	10	92	8	98	9	92	6	92	7	96	2 2	96 90
End of ques. 6–10			←		←		←		←		←	End of cross-exam.	
	92	7	82	9	96	7	90	9	94	14	94	2	98
End of introspection			←		←		←		←		End of ques. by jury	Verdict given	
	96	13	92	10	92	10	96	20	92	3	96	2	92
	Period of quiet										End of introspection		
10	100									14	94		

FIG. 12—*Continued*
Subject H

L								Record Taken During Study	
1		3		9		10			
	92		88	3 2	96 96 96	2 2	108 92 90	3 2 2 2 2 2 2 2 2 2 2 2 2 2	102 102 100 106 100 100 100 90 96 96 96 94 96 96 98
	←		←		←		←		
	96		94	2 2 2 2	106 112 108 110	2 2 2 2 2 2 2 2	96 108 108 118 122 120 108 116		
End of ques. 1–5			←	End of recital			←	Total of 30 mins.	
	106		98	2 2 2	106 106 106	2	112		
End of ques. 6–10			←	End of cross-exam.			←		
	[1]		100	2	100	2	120		
End of introspection			←	Verdict given			←		
			104	2	106	2 2 2	112 106 96		
		Period of quiet			←	End of introspection			
		10	86	2 2	104 100	2	92		

[1] Record incomplete.

H's b. p. record taken during study (see also Fig. 1) seems, on the whole, to show more irregularity, and more tendency to hover about two distinct levels than do his "T" records, while it may be said that *H*'s study record was more consistently and evenly downward in trend than those of any other subject.

FIG. 13

Subject I

T						L								T
4		6		9		5		7		8		10		
	102		110	2 2 2	126 112 106 112		104		124		114	2 2 2 2	106 106 104 108 106	2 3
End of preparation			←		←		←		←		←		←	
16	92	8	92	2 2 2 2 2	122 112 110 110 110	6	114	12	128	7	118	2 2 2 2 2 2	112 116 118 110 110 104	2 2 2
End of ques. 1–5			←	End of recital		End of ques. 1–5			←		←	End of recital		
5	102	7	108	2	116	8	114	8	130	7	116	2	112	2
End of ques. 6–10			←	End of cross-exam.		End of ques. 6–10			←		←	End of cross-exam.		
8	100	7	104	2	118	10	118	12	132	6	120	2	112	2
End of introspection			←	Verdict given		End of introspection			←	End of cross-exam. by jury		Verdict given		
15	104	10	102	2 2 2	116 114 112	13	106	17	120	7	118	2	108	2 5
				End of introspection						Re-direct exam. ends		End of introspection		
				2 2 2	122 120 116					2	124	5	102	5
										Period of quiet				
										17	110			

Subject I.—*I*'s stories were very bare of detail, and told in a prim, precise manner. This was the only subject who preferred truth to deception, and he gave as the reason "that he always found any activity carrying an indifferent feeling tone more pleasant than one stirring up emotion." During

story to bring about a full deceptive attitude, but it constantly tended to do so whenever the fear in the background of consciousness was touched by associations, or crept toward the focus in expression. In short, this curve represents a story told in fear that the witness will be obliged to lie, with a final fear that the single point of deception may have been detected after all.

D. *Interpretation of Individual Results*

Benussi, as above mentioned, made scarcely any attempt to analyze or to psychologically describe the deceptive consciousness. He does report, however, that his subjects found the work of lying hard, disquieting, and unpleasant; and that they introspected "tension of attention, excitement, and discomfort." As reported, this introspection does not seem illuminating, nor does it agree with the introspection of the subjects in this experiment. It will be noted in the individual results reported above, that all subjects, with the exception of Subject *I*, reported the lying more pleasant than telling the truth. Moreover the pleasantness of the whole attitude, or consciousness, seems to depend upon the added interest of the whole proceeding, as an adventure is more pleasant than routine, and seems also to depend upon the success or failure of the attempt to deceive at any particular point. In this, the deceptive consciousness seems to resemble every other complex state of mind, and does not admit pleasant or unpleasant affective tone as a crucial criterion, or even as a consistent constituent. Nor does "tenseness" serve as any better indicator of deception. Seven subjects introspected tension, and four further designated this feeling as "affective tension." These four were asked, at each experiment, to record the height of this feeling and in no single instance did the "tension" climax have any correlation whatsoever with the climax of the b. p. curve. The tension element really consists of what Benussi calls "tension of attention," or, as it would ordinarily be called, concentration of attention on the task before the subject. It has been made clear by experiment in other fields that concentration of attention involves a

certain involuntary setting of the muscles, and very probably general contractions of large groups of these muscles. Yet these same contractions, due to concentration of attention, occur during study, or during the other forms of mental work used in the preliminary intellectual work records, and, in fact, exactly the same sort of concentration with a feeling of "tension" was introspected during several of the "T" records. It is significant that in these "T" records the b. p. did not fall, but remained on an almost exactly even level throughout. Thus we must recognize a certain tonicity of involuntary muscles due to concentrated attention as an almost constant, if not invariable, concomitant of deception; but we must also recognize that the utmost function of such tonicity is to keep the b. p. from falling, and that concentrated attention is by no means peculiar to deception. We must then seek further for the essentially characteristic constituents of deceptive consciousness.

Fear and *anger*, as mentioned above, are the only two emotions which could produce *moderate* increases of b. p. and since the records above show just such moderate but persistent b. p. changes, it would seem probable that one or both emotions constitute the true key to the mental state during lying. It will be noted, from the individual introspection given above, that every subject introspected some complex emotional state containing the element of fear, while many designated the feeling as simple fear. "Feeling of responsibility," "fear of awkward questions," "nervousness as in an exam," "worry," "sneaky feeling," and feeling "flustered" all point inevitably to fear as the common denominator and chief factor of all introspections during deception.

Is *fear*, then, the sole emotional element in the deceptive consciousness? It seems probable that, during a majority of the deception, it is.

Five subjects, however, introspect "irritation" and *anger* at certain points in their false testimony. Outward signs of anger appeared, in all subjects, whenever they were outwitted into betraying themselves under cross-examination, when they gave their case away by careless inconsistencies, and

occasionally when they considered that they were pressed too closely with questions. From the introspection of the subjects in this experiment, then, it plainly appears that fear always, and anger when in immediate danger of detection, are the characteristic emotional factors betraying deception through the b. p.

What, then, is the psychological organization and mode of operation of fear and anger during deception, and what is their relation to the other conscious elements then present? The stimulus to fear is, of course, a central situation mirroring a relation of *danger* between subject and environment. Professor Cannon substantially proved that, with regard to the primary factor of fear,[1] "the natural response is a pattern reaction, like inborn reflexes of low order, such as sneezing, in which impulses flash through peculiarly coöperating neurone groups of the central system, suddenly, unexpectedly, and in a manner not exactly reproducible by volition."[2] That this central response of fear may occur instantly in reaction to sudden danger is a matter of everyday experience, and that ensuing b. p. changes are scarcely less instantaneous is evidenced by records like No. 11 of Subject *I* where, at the telling of a single monosyllabic lie, the b. p. rose 12 mm. in 2 min. If, then, the b. p. response so immediately follows the creation of a dangerous situation, we would expect to find either an initial increase as great as the b. p. elasticity of the individual permitted, with b. p. remaining at this level throughout the deception; or simply a series of sharp isolated rises at each new lie. In a few of the above results we find "L" records exhibiting the last-mentioned tendency, but far more frequently we find the curve designated above as the significant lying curve. Another factor, therefore, must be at work. The introspection indicates that this factor is to be found in the attempted voluntary control of the fear impulse. The attempt took a distinctly individual form in every case, but the common factor in all the methods employed as reported by introspection was the attempted elimi-

[1] And also of pain and anger.

[2] W. B. Cannon, "Interrelations of Emotions," *Am. Jour. Psy.*, XXV., p. 281.

nation of fear from consciousness by a fixation of the attention on the purely intellectual elements of the story. Thus a significant lying curve is a function of the struggle between the involuntary impulse to express fear in response to awareness of danger, and the voluntary focusing of attention to exclude the fear from consciousness. As the ideational elements of the deception become more and more complex, the awareness of danger becomes more and more firmly established in the foreground of consciousness, and, as the stimulus is thus enhanced, the "natural response" of fear becomes stronger and stronger. In some cases the fear impulse probably never entirely breaks away from the restraint imposed by voluntary inhibitions, but in other records we see evidence that, at the *danger* climax, the fear impulse is wholely uncontrolled. The close correlation between the height of the "L" curve and the climax of the intellectual task of deception[1] evidentially substantiates the nature of this danger climax, and points to exactly that gradual return to normal b. p. level which actually appears in the records. This gradual decrease of fear symptoms is due not so much to strengthening of voluntary control, as to decrease in the force of the fear stimulus, *i. e.*, awareness of danger in the case of most subjects. The voluntary control seems to decrease with the necessity for such control and for this reason questions put at the very end of the cross-examination, or by the jury, may cause the fear impulse to run its momentary course unimpeded, betraying itself in short, pronounced b. p. increase. In the same way we may now explain the b. p. behavior when a subject comes to the experiment with the knowledge that he may be obliged to lie if certain crucial questions are asked concerning "guilty" acts. After the first question is safely past, the telling of the truthful story steadily removes the central stimulus to fear, and, correspondingly, the voluntary control is allowed to relax, with the result that if, later, the awareness of danger actually rises to the focus of attention, the resultant fear impulse is unchecked by volition (see record No. 11, subject *J*).

[1] See page 18 above.

Exceptional subjects may, however, retain in consciousness their voluntary inhibitions to fear impulses despite the cessation of those impulses, and in such cases low, *significant* curves of deception take the place of short, sharp, isolated rises.

"Fear is a reaction aroused by the same objects that arouse ferocity," says James ". . . the question which of the two impulses we shall follow is usually decided by some one of those *collateral circumstances* of the particular case."[1]

I would suggest that the collateral circumstance which always turns fear into rage is the occupation of the focus of attention by the awareness that there is no escape from the danger impending. This may occur, during deception, either by some sudden betrayal, or by the victory of fear in winning its way to conscious motor expression and so betraying the lie. In either case, the anger impulse supersedes the central fear reflex, and rage is registered in consciousness. But, since both emotions are expressed through the sympathetic nervous system, the visceral changes which have been taking place during fear continue during rage, and the b. p. level will merely depend upon the strength of the anger impulse aroused. If (as with Subject A, Record No. 5) the anger is slight, and of short duration, it will almost immediately allow the b. p. to drop as during a "T" record; if, however (as with Subject *E*, Record No. 10, No. 11), the anger felt is, at least momentarily, stronger than the fear previously in consciousness, a rise in b. p. will take place. In a situation such as that produced by deception in real life, it is very probable that rage at detection or at self-betrayal would be far more intense and of much longer duration than under the artificial conditions of the laboratory. Finally, there is little possibility that, in this experiment, emotions other than rage and fear were aroused to a sufficient degree of intensity during deception to break over into the sympathetic nerve channels and so affect the b. p., and probably in actual experience such contingencies would be of almost equally rare occurrence.

It may be noted at this point that, if we resolve the crucial

[1] "Principles of Psychology," II, p. 415.

deceptive factors into fear and rage, we have a seeming anomaly in the almost unanimous introspective report that deception was *pleasant*. But it was the *whole experience* of the deception that the subjects found pleasant, not the isolated fear, and perhaps not the actual moments when the fear was recognized as such. Men prefer bluffing at poker to playing a conservative hand, and the explanation would seem to be that excitement is more pleasant than quietude, any emotional experience being preferred, perhaps, to a purely intellectual activity. Where, as in actual court work, life and happiness might hang on the success of a deception, it is much more doubtful whether the whole situation during deception would be more pleasant than while telling the truth. Probably, in such cases, both would be disagreeable.

Whether the experience of fear *can* be pleasant per se is a more difficult question, which need not be decided here. The popularity of such amusements as "scenic railways," the sole attraction of which lies in the *fright* on the steep inclines, suggests that fear may be pleasant—at least retrospectively.

Of the "T" records, 76 per cent. show downward b. p. curves, 62 per cent. showing final return of b. p. toward its initial level. On the whole this unlooked-for result seems too persistent to be entirely disregarded. Certainly no significant emotional element appears in the "T" introspection, and the only common factor there seems to be a very general mental and physical feeling of passivity. Thus the effort necessary to maintain attention on the task in hand is prominent in consciousness, and the subject remains indifferent to mistakes, or failures of memory. A general passive mental attitude of this sort might affect the b. p. in two ways: (1) By general relaxation of all muscles, and consequent lack of cardio-accelerator impulses, with the possibility of actual inhibitions at the accelerator center; (2) by vaso-dilations throughout the body such as occur during sleep,[1] with consequent lowering of diastolic b. p. and indirect reduction in systolic pressure. Such influences would, in short, tend to remove several of the ordinarily acting physiological factors

[1] See Howell's "Physiology," 5th ed., pp. 255ff.

which determine the subject's average b. p.; and would not prevail against any positive influences which might be suddenly introduced. That the uniformity of "T" curves is due to some such general negative influence is indicated by (1) the fact that it does not take effect in 24 per cent. of the "T" records; (2) the fact that it is clearly overcome in a few records by positive influences such as muscular contractions; (3) the fact that the average drop in b. p. during "T" records was only 8 mm., as contrasted to an average increase of 16 mm. for all "L" records, and (4) the fact that the "T" curves are much more irregular than the "L" curves, and subject to inexplicable variations as large as that record's minimum decrease. Thus it appears fairly certain that the downward "T" curve is a function of one or more general, negative influences, but it is much more difficult (and perhaps less profitable) to discover which of the great possible numbers of such influences determine the result recorded in the "T" records.

E. Judgments of Experimenter and Jurymen

A total of 107 records were taken in this experiment. 10 of these records were "T and L" records, and 3 of the "T" records were influenced by muscular contractions. The experimenter, basing his judgment entirely upon the b. p. behavior, made 103 correct judgments and 4 erroneous ones. Of the latter, 3 mistakes were made on "T and L" records, and 1 mistake on a "T" record influenced by muscular contractions. The b. p. judgments were, then, 96 per cent. correct.

Benussi reported that he found two distinct classes of liars divided upon the basis of their success in deceiving his "trained observers." He further reported that the breathing of the successful liars was more modified during deception than was that of those unsuccessful at deception. No such result was obtained in this experiment, however, as the following table will show. Only the judgments of those men who sat regularly in the juries and whose judgments were therefore susceptible of comparative study, are recorded in Fig. 15.

FIG. 15

Jurors' Judgments

Subjects...	*A*		*B*		*C*		*D*		*E*		*F*		*G*		*H*		*I*		*J*		% R.
Jurymen...	R.	W.	R.	W.	R.	W.	R.	W.	R.	W.	R.	W.	R.	W.	R.	W.	R.	W.	R.	W.	
1												1							2	4	.28
2																			4	3	.57
3					5	7															.41
4					7	3			5	3							5	2			.68
5													6	4							.60
6		1											2	7							.20
7							3	7											1	3	.28
8							4	1													.80
9							2	1			5	1							3	1	.76
10	2	1											2		6	4					.66
11			1	1							3	4			1	2	1	2			.40
12											4	4									.50
13	1	3							2	2							2	4			.35
14																	2	5			.28
15		4	1	5																	.10
16			4	1													3	2			.70
17			4	3						2											.44
18	4	1			2																.85
19	2								5	1	1	4									.61
20									2	6		3									.18
% W.	.53		.50		.42		.50		.50		.57		.52		.46		.54		.52		

Note.—"R." = Right and W. = Wrong, judgments. Per cent. R. = Per cent. of juror's correct judgments. Per cent. W = Per cent. of subject's successful deceptions of the jurors.

It is clearly evident, from the above table, that the jurors, not the subjects, are the ones to be divided into successful and unsuccessful classes. 7 of the subjects were from 50 per cent. to 57 per cent. successful in deceiving the jury, but these percentages are obviously too low to be significant. Jurors 4, 9 and 10, on the other hand were clearly successful at "sizing up" the subject, all three being consistently successful in judgments made upon three different subjects. Other jurors, such as 1, 6 and 20, are clearly poor judges of men, and, although the classes of successful and unsuccessful jurors approach within 20 per cent. of each other, only No. 12 is squarely on that neutral line of 50 per cent. about which the percentages of the subjects so consistently hover.

F. Series D, Checking Series.

This series followed exactly the method of Series *B*, an extra story being prepared and used throughout the series. A

juror was selected without any warning, and requested to take the subject's place for that day, while the subject took his place in the jury. The chief importance of this series lies in testing the practical value of the b. p. record as an indicator of deception. These subjects could have had no fear or other emotion associated with previous experiences as subjects in that experiment, and were in exactly the position of any naïve witnesses who might be called upon anywhere to testify. It will be noted that 5 clearly *significant* "L" records were obtained, that the "T" records were all of a persistent downward tendency and that, when juror 9 lied a little in answer to the last question, he was immediately betrayed by the b. p. All b. p. judgments in this series were correct.

FIG. 16

Extra Story, Series D

T						L										T and L	
1		2		3		4		5		6		7		8		9	
	116		108		118		126		110		130		118		124		112
End of preparation			←		←		←		←		←		←		←		←
4	114	6	98	4	110	3	140	10	122	8	142	10	134	10	138	6	108
End of ques. 1–5			←		←		←		←		←		←		←		←
7	112	7	102	6	108	8	144	7	126	6	128	6	126	8	140	7	106
End of ques. 5–10			←		←		←		←		←		←		←		←
12	116	11	106	9	116	8	138	6	118	8 2	130 122	10	116	9	128	9	118[1]
End of cross-exam. by experimenter			←		←		←		←		←	End of cross-exam.			←	End of cross-exam. by experimenter	
2	116	1	102	3	120	2	140	3	120	2 6	132 128	15	108	11	110	1	112
End of cross-exam. by jury			←		←		←		←		←						←
6	114	12	100	12	118	12	124	3	120	6	118					14	102

[1] Lied at Ques. 10.

With reference to the practical application of the b. p. test, a final individual experiment is of interest. Mr. Dewey, who had witnessed several of the experiments, proposed to tell two stories of his actions on a certain afternoon, both stories to be objectively true, but one set of actions to be those which he performed during another afternoon than the one selected. The curves in Fig. 17 show the result. The "L" curve, while not great in height, is perfect in form as a significant deception curve, and contrasts sharply with the truth curve which,

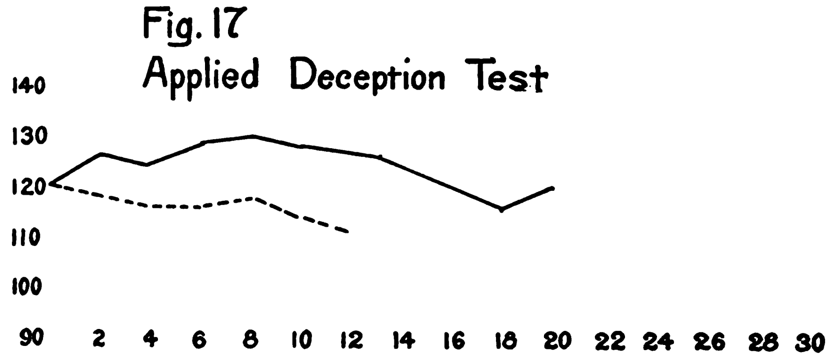

FIG. 17.

starting at identically the same b. p. level, drops slowly and evenly 8 mm. The experimenter passed over, in writing, the correct judgment at the end of the fourth b. p. reading of the second curve.

5. Conclusions

1. The behavior of the b. p. does not act as the least indicator of the objective validity of the story told by any witness, but it constitutes a practically infallible test of the *consciousness of an attitude of deception.* Mere awareness of a mistake, even if the mistake is uncorrected, or the mere addition of trifling details, even if the subject is conscious of such additions, will not constitute that mental situation which is the necessary stimulus to fear, and will not, therefore, cause the b. p. to rise.

2. The *significant* curve of deception differentiates a story the foundation of which is false from a story mostly

true, but containing one or two substantial lies. The sudden sharp, short rises of b. p. betray these substantial lies in an otherwise true story. It seems probable that, if a truthful witness became violently angry at some chance question of the examiner, or if he suddenly saw his worst enemy glaring at him, gun in hand, in the court-room, his b. p. would suffer a short, abrupt rise, but if such extreme outside influences are avoided, all major b. p. modifications would seem to depend upon the deception elements of the story itself.

3. The b. p. record during testimony might be made practical use of as an indicator of deception if the test embodied the following features:

(*a*) Two records must be taken as in the test on Mr. Dewey, the story told during one record being truth within the knowledge of the examiner.

(*b*) The examination should be private, with carefully controlled conditions, and means at hand for recording involuntary movements, muscular contractions, and sudden or suppressed laughter.

(*c*) The record should be interpreted by a psychologist experienced in this particular line, and should be scrutinized with careful reference to the construction and subject matter of the story, the record of the manner and muscular contractions of the witness, and above all it should be compared minutely with the record known to be symptomatic of that individual's consciousness while telling the truth.[1]

[1] The writer expresses his gratitude to Professor Münsterberg and Professor Herbert S. Langfeld, to his assistants, E. H. Marston and T. Ramsdell, and to the Harvard men who served as jurors.

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REACTION-TIME SYMPTOMS OF DECEPTION

BY

WILLIAM M. MARSTON

I. PROBLEM

The experiment hereinafter reported was performed in the Harvard Psychological Laboratory during the Academic Year 1913–1914. At that time the writer of the present article, at the suggestion and under the direction of Professor Hugo Münsterberg, began experiment upon what was then planned to be a series of psycho-physiological problems in the field of legal testimony. The first psycho-legal problem to be approached was the investigation of the psycho-physiological symptoms of the deceptive consciousness. The systolic blood pressure symptoms of lying were reported upon by the present writer in the JOURNAL OF EXPERIMENTAL PSYCHOLOGY for April, 1917. Since that time considerable work has been done by the present writer and others upon deception tests in connection with the various psychological tasks undertaken for the Federal Government during the recent war. The present writer, in returning to the further laboratory investigation of the deceptive consciousness, is led to believe in the light of above-mentioned Army deception test experiments, that the practical value of psychological studies in this field lies almost wholly in a complete and comprehensive scientific discovery and analysis of all the psychological symptoms of deception rather than in attempted use of one isolated set of these symptoms for detection of deception on the part of witnesses or criminals.

A glance at the legal situation becomes necessary in pointing the practical problem. Mr. Bielaski, active head of the Bureau of Criminal Investigation for the Department of Justice during the war, expressed the opinion that the future of all deception tests lies in the possibility of their introduc-

tion as a basis for expert testimony, just as the various neuro-psychiatric tests are now used as the basis for testimony by alienists in cases of alleged insanity. In order to qualify a psychologist as an expert upon deception a psychological "patent medicine" is not sufficient. The court must be convinced that a sufficiently sound and fundamentally scientific knowledge of all the psychological symptoms of deception are available to enable the alleged expert, not only to give his opinion to the jury, but also to answer hypothetical questions put to him under cross examination, showing upon what state of psycho-physiological fact said opinion has been based. A "patent medicine" is a secret formula known only to the owner of the copyright and to the United States Bureau of Patents. The maximum value of many such proprietary remedies lies in the suggestion exercised upon the mind of the patient by the mystery of the contents, with the resulting apparently miraculous effect of the remedy upon the disease. It is to avoid exactly this suggestion of the miraculous or mysterious upon the minds of juries that our present trial systems demand, not only expert testimony, but the qualification of the expert by proof of the existence of a commonly known and recognized body of scientific fact upon which the expert bases his opinion. Thus, it is extremely doubtful whether a single deception test, such as the association reaction time test, the Benussi breathing test, or the systolic blood pressure test, would be admitted as a basis for expert testimony in court without a broader foundation of psychological fact than is at present available. In short, the court will not be satisfied to be assured by psychological authorities that this or that deception test does detect deception; many medical authorities might similarly testify as to certain patent medicines. The courts, through long experience in the analysis of testimony, are not looking for patent medicines; they want a free and common knowledge of all the ingredients.

We must, therefore, seek objective, quantitative measurements of the psycho-physiological symptoms of the deceptive consciousness. If a psychologist has to go on the stand and

testify that, in his opinion, the alibi of a defendant indicted for first degree murder is a lie, it seems obvious that no qualitative analysis of the symptoms during testimony will be sufficient. The quantitative analysis may indeed show only relative difference or significance but, nevertheless, quantitative measurements are in themselves definite and susceptible of common knowledge. The immediate problem of the experiment here reported was to study the reaction times of the subject during deception. Analysis of reaction time association experiments and tests reported in the literature shows that judgments have been based jointly upon the qualitative and quantitative aspects of the results. The qualitative aspect of these tests lies in the association reaction words, and the quantitative in the reaction times recorded. Our problem in the present experiment was to eliminate the words and isolate, if possible, the quantitative measurements. In all previously reported tests no unanimity of qualitative results has been reported but, on the other hand, so great a unanimity of quantitative results has generally been found that psychologists seem to have accepted as established the thesis that deceptive emotions tend to increase both association reaction times and mean variations. To test this thesis is the particular problem of the present experiment.

II. Method

Ten different subjects were used in the principal experiment; six subjects were graduate students of psychology who might properly be designated as trained subjects, and four were undergraduates of competent general psychological training but with no specialized laboratory experience. The method adopted purposed the exclusion of all mental elements extraneous to the interaction of deceptive consciousness and reaction times.

Series A.—Pasteboard index cards were prepared, carrying upon one side two columns of words, ten words to the column, printed in parallel columns on one side of the card. The cards were placed blank side up before the subject who was instructed at a given signal to turn over the card and give

an association word with each word in one of the lists on the card as fast as he could. The two columns of words on a card were called L. and R. (left and right) and the two opposite directions in reading each list were called U. and D. (up and down). The cards were presented to the subject in groups of eight. Before giving the signal for the subject to turn over each card, the experimenter instructed the subject to choose either the right or the left column on that card and to proceed through that column either up or down. In any four cards out of each group of eight the subject was to obey the experimenter's instructions and in the other four out of that group the subject was to reverse instructions by taking the opposite list and proceeding in the opposite direction. The subject was to deceive the experimenter as to the identity of the cards where instructions were reversed by proceeding to make his associations with the list on these cards as rapidly as on the cards where he obeyed instructions. The experimenter, of course, did not know which of the lists were done truthfully according to direction, and which were done deceitfully against direction. The truthful cards were called C. (correct) and the deceitful cards O. (opposite). At the end of each group of eight cards the experimenter attempted to judge by means of the reaction times alone which four lists the subject had performed C. and which four O. The reaction times were taken with a stop watch operated by a magnet; the circuit being closed first by a Dunlap voice key, but later, when this proved unsatisfactory, by a tapping signal of the subject.

Series B.—After preliminary series *A* had been run off, the quantitative aspect of deceptive reaction times was still further isolated by substituting lists of two digit numbers for the words upon the cards presented to the subject. In addition to the directions of L. or R. and U. or D. which the subject might obey or reverse the experimenter now instructed the subject to add or subtract from each number on the list two, three, four or five. If the subject were determined to deceive upon any card he not only chose the column and direction opposite to the experimenter's instructions, but also

added when instructed to subtract and vice versa. By this method it was intended to eliminate all meaning from the symbols dealt with in the subject's mental process, and to isolate as far as possible, the factor of deceiving the experimenter by uniformity in the reactions required of the subject. Time was allowed between the instruction and the starting signal for the subject to perform the central process of reversing the instructions, and the watch was started simultaneously with pronouncement by the subject of his first result; so that reaction times recorded actually included the time used in nine additions or subtractions. The interest of every subject but one was intrigued to a high degree (as reported in introspection and as evidenced by behavior during the experiment). This condition was established largely by the aggressiveness of the experimenter and the consequent keen desire of the subjects to "fool" or beat him at the game of deception.

A total of eleven series of tests were recorded, one of the ten subjects taking part in two series. Each series of tests exclusive of preliminary series *A* included 40 sets, eight lists to the set, as above described, four sets being run through in about an hour. Every subject reported positively that he attained the desired attitude of deception and each number list was given four times, each time with a different digit to subtract or add, in order to exclude as far as possible any chance influence of combinations of figures peculiarly difficult to any individual subject. In the principal experiment total averages were made separately for all O. reaction times and for all C. reaction times of each subject. Comparative studies and further analyses of these averages were made, as will appear in detail hereafter.

III. Results

The results of this experiment disclose two distinct types of reaction time behavior during deception. First, the positive type was clearly evident in four out of ten subjects. This is the type, hitherto believed to be practically universal, which is characterized by increased length of reaction times during deception, with accompanying increase of mean varia-

tions. Secondly, a negative type manifested itself with equal distinctness in three out of ten subjects. In the cases of these three subjects reaction times during deception appeared to be consistently shortened with, however, no consistent accompanying effect upon mean variations. Thirdly, a mixed group appeared in the cases of four subjects who completely shifted from positive to negative types on different days, behaving consistently within the positive or negative types on each single day's work.

A. Positive Type

No detailed study of results to prove the existence of this type will be necessary in the present experiment, since the positive deceptive type is probably most usual, and has been sufficiently established by all previous experiments reported within the reaction-time field. Table I. gives a summary and analysis of the quantitative reaction time results obtained for the four positive type subjects in this experiment.

TABLE I

Subject	Total Average "C" Reaction Times	Total Average "O" Reaction Times	Positive Difference Between "C" and "O" Averages	M. V. "C" Reaction Times	M. V. "O" Reaction Times	Positive Difference Between "C" and "O" M. V.	Probable Error	Average Percentage Correct Judgments
A.....	10.8344	14.546	3.7116	1.572	1.8331	1.8331	.1906	81.89
D.....	4.6592	5.5605	.9013	.5192	.5882	.069	.0568	81.57
E......	7.3848	8.5181	1.1333	.7727	.8118	.0391	.09072	67.42
S......	10.48	12.2771	1.797	1.598	2.402	.804	.2163	75.00

Hitherto it has generally been assumed in the diagnostic experiments that the m.v. for the critical or deceptive words will be greater than that for the non-critical. In the positive type the results of this experiment substantiate those of the diagnostic investigators. In the case of all four of the subjects of positive type the average m.v. for the deceitful reaction times exceeds the average m.v. for the truthful measurements by a margin that is probably significant. This state of fact also has been well established in the literature.

The introspections of positive type subjects, when analysed to the lowest common denominator, show *fear* appear-

ing in many guises, and this report of fear at crucial points does not appear in the introspection of the negative type subjects. One subject is "afraid that he will obey the instructions of the experimenter rather than his own opposite setting." Another subject feels "withdrawal from the experimenter while attempting to deceive," "feeling of nausea," "shrinking feeling." Both seem palpable expressions of a real fear content. "Tension" is also introspected. This "tension" does not seem to be that indefinable attribute of feeling tone postulated by Wundt, but consists of a very concrete and often localized muscular tension, sometimes in the feet, in clenching of the hands, in throat muscles, breathing muscles, muscles of the mouth, and especially at the diaphragm. One subject described this tension as "a restraint across the ribs, like indigestion." "Excitement" is a second introspective element reported solely by positive type subjects. It is variously described as "nervousness," "exhaustive excitement," and "general emotionality." I shall defer any discussion of the exact nature of this element to the consideration in a later report of blood pressure results obtained in war work which seem to have significant bearing upon the much disputed tridimentional theory of Wundt. Let me anticipate, however, by stating that I take this excitement to be an awareness of the visceral expression of any of the three major emotions expressed in the sympathetic division of the autonomic nervous system or of any other emotion which has flowed over into that system. The confusion of motor impulses which is very evident in the behavior of positive type subjects, is described introspectively as "conflict of more processes during the deceptive lists." Auditory, kinæsthetic, and visual imagery of the experimenter's instructions recur in consciousness and the mind is reported to "become blank." Subjects introspect that, in the truthful reactions, the experimenter's directions reinforce the motor setting while, in the deceptive reactions, above mentioned, imagery tends to break it up. They "forget whether they are adding or subtracting."

The behavior of the positive type subjects, which was

carefully noted upon each record by the experimenter, tends to bear out the introspection of the positive type as reported above. The flush, traditionally indicative of guilt, is almost always present with the positive type. Frequent obvious confusions and embarrassments are noted together with evidences in facial expression of great effort, which effort, nevertheless, proves unsuccessful in precluding numerous clearly manifested inhibitions and interferences of motor impulses.

What, then, is the mechanism by which positive type reaction times are delayed? Jung and his school unanimously ascribed the increase of reaction times to emotion, and, from the above reported results, emotion of one sort or another would certainly seem to be the prime cause. We may go a step further, I believe, with the data in hand, and postulate *fear* variously expressed and always more or less Freudianly concealed, even in the introspection, as the prime cause underlying delayed reaction times in the positive type subjects. The witness, unable to concentrate because of the inevitable physiological expressions of his fear, becomes more and more introspectively aware of the fear content itself, and proceeds to exert great effort to suppress this fear, yet, since the only method of successfully suppressing emotion is to eliminate this emotion from the focus of consciousness by concentration, such increased effort only tends to bring intellectual and motor processes into consciousness in addition to the fear content already present. At this step, introspective awareness of the then conscious difficulties of performing the task in hand act as a new and adequate stimulus to a new set of fear reactions (which, of course, constantly enhance themselves through the adrenal autonomic mechanism). The final consequence, which is of course inevitable in any intellectual task performed under distraction, is a conflict and confusion of motor impulses directed to the work in hand, with prolonged inhibitions as the final step. In short, the positive type subject, engaged in the game of deceit, makes a tremendous effort to win, but, like the beginner at golf or tennis, he is unable to concentrate his attention upon the end to be

attained, with the result that consciousness of muscular movements and the general modus operandi precludes success and he misses the stroke. The positive type subject is the unsuccessful liar in the detection of whom trial lawyers and jurists have delighted through the centuries.[1]

B. Negative Type

Table II. shows that, taking the analysis of averages just as it stands, the negative type is precisely as clean cut and well-marked as the positive.

TABLE II

Subject	Total Average "C" Reaction Times	Total Average "O" Reaction Times	Negative Difference Between "C" and "O" Averages	M. V. "C" Reaction Times	M. V. "O" Reaction Times	Negative Difference Between "C" and "O" M. V.	Probable Error	Average Percentage Correct Judgments
C......	9.85147	9.48308	.36839	1.6294	1.5693	.0600	.1831	78.67
G......	11.4955	10.7975	.6980	1.1425	1.1764	.0339	.1837	75.00
K......	8.4701	7.9361	.5341	.8794	.5716	.3078	.1017	75.00

A careful study of the individual data, together with a comparison of the averages for different days, shows, however, that the balance is much more unsettled. The negative attitude seems much harder to maintain. These conclusions are formed principally from the greater relative number of reversals of reaction time behavior; although unlike the reaction times of the mixed group no two of these reversals ever occur together, and in no instance does a reversal of type persist during an entire day's work. Transferring our attention, for the moment, from laboratory experimentation to ordinary everyday experience, I believe it will be commonly recognized that there are individuals among our acquaintance who can lie faster and more fluently than they can tell the truth. This type of liar never has been discovered in association reaction time experiments as reported in the literature, probably for the very simple reason that such individuals were wiped out in the general averages and left unaccounted

[1] It is of interest to note that the positive type give the most extreme and erratic blood pressure changes; but exhibit the least marked breathing changes, according to Benussi.

for when experimenters reported as their final net result a certain percentage of cases successfully detected by association reaction time tests. Indeed, in both the experiments of Washburn and Leach[1] and Yerkes and Berry[2] close inspection of the quantitative results will reveal instances of this unrecognized negative type. Under the conditions of the present experiment it seems clear that, the confusing and complicating factors of the equation having been eliminated, and the bare quantitative reaction time behavior alone having been retained, the negative type appears with unmistakable definiteness. Indeed, during the latter part of the experimentation, after this type had begun to manifest itself, the experimenter was able to bring his percentage of correct judgments from 32 per cent. to 75 per cent. in the cases of subjects *G* and *H*, merely by picking the faster reaction time lists as deceptive.

In two of the three subjects of negative type it will be noted from Table II. that negative m.v. differences of .06 and .3078 appear. Subject *G*, on the other hand, shows a positive m.v. difference of .0339. Just what value is to be placed upon these somewhat equivocal m.v. differences is rather problematical. In general, it would seem to show that while the positive type generally tends to show positive m.v. differences, the negative type is not so definitely correlated with any particular m.v. behavior.

Subjects of the negative type give very little introspective data indicative of fear. Occasionally the subject reported "worry" or some similar emotional state of which fear is probably an ingredient, but these elements were always reported in the fringe of consciousness only. Anger, on the other hand, was frequently introspected and seemed to be aroused in negative type subjects by the least difficulty in arithmetical processes during the deceptive lists. Negative type subjects reported that they felt a certain "tension" coming on (especially in the breathing muscles which would seem to bear on Benussi's results); but that they felt that

[1] *American Journal of Psychology*, 1910, pages 63–67.

[2] *American Journal of Psychology*, 1909, pages 22–37.

this tension would interfere with the speed of their reaction times and they, in consequence, were able voluntarily to relax. It is this relaxation which the positive type of subject never reports himself able to accomplish. No negative type subject reported "excitement." All, however, reported a definite "plan." This element is reported as persisting in the consciousness of negative type subjects both as a motor organization and as an intellectually retained concept and is an element entirely lacking in the deceptive consciousness of the positive type as reported. Both motor setting and concept are relegated to the fringe of consciousness when actual arithmetical processes commence, but certainly both seem to co-exist with the other elements during the entire deceptive consciousness. The plan seems to include: (*a*) plan to go slower on the truthful reactions and to welcome any difficulties which will lengthen these reaction times; (*b*) plan to relax muscular tension whenever it reaches consciousness; (*c*) plan to keep just within the maximum speed during deceptive reactions but not to try to exceed this rate lest subject should "break into inhibitions." In this connection it is interesting to note that both positive and negative types believed that deceptive reactions were much slower than their truthful ones. The negative type subjects reported a "tremendous feeling of effort with occasional necessity for voluntarily inhibiting persistent imagery of one sort or another which may disturb attention." This voluntary inhibition seems to be successful with the negative type as evidenced in the quantitative results.

The behavior of the negative type subjects, as noted by the experimenter, was almost wholly calm, confident, and showed a high degree of intellectual concentration. The guilty flush was almost altogether absent and the manner of the subject was usually more convincing upon deceptive lists than upon truthful ones. Occasionally inhibitions were noted but 95 per cent. of all such inhibitions occurred during the truthful lists, and as reported by the subjects' introspection seemed to be welcomed by the witness and even voluntarily prolonged. The experimenter frequently observed, however,

the tendency in negative type subjects to avoid the experimenter's eyes; and this symptom suggests that fear is present with subjects of the negative type also, although such subjects are successful in preventing the emotion from emerging into the focus of attention.

In the negative type we find the successful liar. The psycho-physiological mechanism by which reaction times are delayed with subjects of the positive type is voluntarily controlled and stopped at its inception by negative type witnesses, with the result that we have the intellectual task of sustaining deception at the very center of attention. The mind then being concentrated upon the end to be gained is able to use all its ingenuity and skill in deceiving the experimenter, and, since no mind is capable of exact judgments of time over a long series of reactions, the only flaw in the negative type's efforts at deception lies in said efforts being *too successful*. This, however, is a reaction time difference of much more unstable and nicely balanced character than is the positive type difference, since any temporary discomfiture of the negative type during deception or any chance impairment of general mental fitness will probably delay the deceptive reactions just enough to make them practically indistinguishable from truthful measurements. The negative type subject then is the one who, through the centuries, has had the joke on lawyers and juries.[1]

Table III. shows at a glance why further analysis of two apparently positive type subjects and two as obviously negative type subjects is deemed necessary. Each row of figures on Table III. represents one day's work and it will be seen that the characteristic which differentiates these four subjects from the general positive and negative types is the complete reversal of reaction-time behavior from one type to the other during an entire day's work. It is, of course, to be expected that occasional reversals of reaction-time behavior should occur and where such reversals occur normally scat-

[1] The blood pressure symptoms of the negative type show a more even curve but with just as great or even greater net rise in b.p. than is found in the positive type. According to Benussi the breathing of the successful liar shows greater modification than that of the unsuccessful.

tered through the general tests no significance could be attached thereto; yet, when the subject so completely shifts from one type to the other, that, after the first series of tests on the reversal days, the experimenter was able to attain a percentage of over 90 in his correct judgments of deception by postulating a shift in type, it seems valuable to further subdivide positive and negative type individuals who are subject to these clear-cut reversals. For the sake of convenience in reporting this further analysis the writer has called

C. Mixed Group

TABLE III

Mixed Group											
Positive						Negative					
Subject B			Subject F			Subject C			Subject I		
Number Sets per Day	Number Positive	Number Negative	Number Sets per Day	Number Positive	Number Negative	Number Sets per Day	Number Positive	Number Negative	Number Sets per Day	Number Positive	Number Negative
2	2	0	3	1	2	2	0	2	*2	2	0
2	2	0	4	4	0	4	0	4	*4	4	0
2	2	0	4	3	1	4	2	2	*6	6	0
*2	0	2	4	3	1	*3	2	1	*3	3	0
2	2	0	*4	1	3	4	1	3	4	1	3
*2	0	2	4	4	0	4	1	3	4	0	4
4	3	1	4	4	0	*4	3	1	4	1	3
*6	0	6	3	3	0	4	0	4	4	0	4
4	4	0	3	3	0	*3	2	1	4	0	4
4	3	1	*4	0	4	*4	4	0	4	1	3
8	8	0	*6	0	6	4	0	4	4	0	4
6	6	0	4	4	0	3	0	3	4	0	4

these four subjects a mixed group, although it should be understood that the name is not intended in itself to carry any significance. When the above-mentioned reversals are checked up with the behavior of the subject on reversal days, we find that on negative reversal days of positive subjects the physical and mental condition of the subject seems to have been exceptionally good, with frequent evidences of the subject's being in good spirits, or particularly happy over some occurrence in his personal affairs. Also, behavior on these reversal days seems correspondingly to

shift to negative type behavior, carrying with it a change in the general tone of the introspection. In short, it appears that when these positive type subjects were in an exceptionally favorable physical and mental condition they were able to attain the successful poise of the negative type in deception. Again, it will be noted in Table III. that subject *I* shows a marked positive reaction time type for the first four days of experimentation, and that thereafter he attained the negative speed of reaction and held it to the finish. This seems clearly to suggest preliminary practice with resultant skill in the game of deception. It is also to be noted that subject *C* reported himself "fatigued or physically tired" on every day when his reaction times lapsed into the positive type.

TABLE IV

Mixed Group					
Type		Positive		Negative	
Subject		B	F	C	I
Totals of reversal days	Average "C." lists	9.0781	13.893	8.175	6.658
	Average "O." lists	7.8844	12.1135	8.76875	8.7709
	Difference	−1.1938	−1.7807	+ .5938	+2.1129
	M. V. "C." lists	1.1889	1.95	.972	.996
	M. V. "O." lists	.9718	1.1188	2.322	2.123
	Difference	− .2171	− .8312	+1.350	+1.127
Totals without reversal days	Average "C." lists	7.308	11.848	9.5416	6.7125
	Average "O." lists	8.492	13.307	8.3887	5.4125
	Difference	+1.1839	+1.459	−1.1529	−1.3
	M. V. "C." lists	.8733	1.502	1.2016	1.0764
	M. V. "O." lists	1.2539	2.427	1.1693	1.0292
	Difference	+ .3806	+ .925	− .0322	− .0472
Total of all days	Average "C." lists	7.7014	12.096	9.38319	6.903
	Average "O." lists	8.3569	13.162	8.43219	6.234
	Difference	+ .6556	+1.066	− .951	− .669
	M. V. "C." lists	.9434	1.556	1.1754	1.0764
	M. V. "O." lists	1.1912	2.269	1.3011	1.0292
	Difference	+ .2912	+ .713	+ .1257	− .0472
	Average per cent. correct judgments	81.61	78.03	80	78.83
	Probable error	.1061	.1828	.1340	.1326

Table IV. shows evidence, in further analysis of reversal day averages as compared with total type averages for above four subjects, tending to indicate sharp contrasts in quantitative measurement between reversal day and general type

reactions. Of course these reversal averages could be left buried in the general quantitative average, but above further analysis shows the sharp distinctness of the averages of the unified groups when same are separated. In general the study of Table IV. is intended to contrast the two markedly opposite statistical tendencies of the reaction times of mixed group subjects. It may be noted that both positively inclined subjects in the mixed group show positive m.v. differences for their total average m.v., while negative m.v. differences appear for the negative reversal groups. The two negatively inclined subjects of the mixed group, moreover, show negative total m.v. differences, with positive m.v. differences for positive reversal groups. The writer does not attach any great significance to this m.v. as a criterion, since the actual differences were very small and would be very hard to follow up in any practical test. It is of interest, however, to note that, other factors being equal, the m.v. differences seem to follow the general type of the subject.

It appears to the writer that the mixed group probably takes in the whole middle range of liars. Only very good liars succeed in keeping every single day's work up to negative type deception speed; and most negative type liars probably have their "off days" when they are unable to command their attention and mind to the high degree necessary to shorten the reaction times during deception. On the other hand it is a very poor liar who is never able to attain sufficient concentration to throw himself for a day into the class of successful deceivers. It would seem to the writer that, after practice in deception was eliminated, the predominant type of any subject could easily be detected by a sufficiently long series of measurements; and it may be said that the experimenter in the present experiment, after once determining the general type of a subject, found reversal days exceptionally productive of accurate judgments because, during such reversals, the unnatural tendency of the subject seemed to be exaggerated and very consistent.

Psycho-legal Conclusions

It seems to the writer that this experiment has added one stone to the foundation necessary to qualify the psychological expert on deception before the court. The district attorney points to the flushed face and obvious confusion of a witness for the defense; but our psychologist is put on the stand to testify that he has proved the defendant to be of negative deception type. Attorney for the defense then can place before the jury the psychological fact that, were a negative type witness lying, no such flushed face and obvious confusion would appear, for the negative type witness is the gifted liar who would be expected to exhibit much less confusion were he lying than if he were telling the truth. Or our expert may succeed in discrediting the star witness for the state solely because said witness, being a subject of negative reaction time type, was oversuccessful in going through a terrific cross examination with unbroken rapidity and consistency. The stress and high tension of the layman's mildest experience in court, either as witness or as a criminal defendant, should, I believe, tend to keep the individual well within his predominant type; yet our knowedge that there exists a mixed group subject to sharp reversals of reaction time behavior, should lead the boldest psychological expert to check up his reaction time results with other test data before venturing an opinion in court. In any event, if psychological authority can succeed generally in disabusing the minds of the average jury of the conviction that the slightest hesitation, delay, or confusion in the testimony of any witness is indicative of deception, or that rapid and clever answers in cross-examination are indubitable earmarks of a white conscience, a constructive development in the weighing of evidence should be effected in an innumerable number of trial cases.

AESTHETIC UNITY

An Investigation into the Conditions that Favor the Apperception of a Manifold as a Unit[1]

By Margaret Otis

The principle of unity is fundamental for any theory of aesthetics, for the harmonious union of the many in the one is necessarily involved in all aesthetic experience. The treatment of the aesthetic experience as an independent and fundamental life value is found presented by Münsterberg,[2] who places the inner agreement of meaning in the manifold as essential for the beautiful object. Aesthetic unity as thus understood is the subject of the present investigation, for it is a study of the inner agreement and disagreement of the various factors that go to make a work of art.

As a problem in experimental psychology the perceptive process is the one immediately concerned. In all our conscious experience we are constantly unifying various elements into wholes. Given a mass of lines, dots, or material of any kind, a selective process occurs and out of the mass groups are formed, the "higher units"[3] of perception. The dot figure of McDougall[4] well illustrates this process, also the lines and squares used by F. Schumann[5] in his investigation of the process of visual perception. The problem of the attention is involved as in the interpretation of equivocal figures and puzzle pictures, for example, the six-pointed star of James[6] that may be perceived as two superposed triangles, or as a hexagon with six small triangles, one attached to each side. Titchener[7] refers to cases of puzzle pictures in discussing his theory of the two levels of attention.

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[1] The problem was suggested by the late Professor Münsterberg and was conducted in the Harvard Psychological Laboratory during the academic year 1909-1910. There were 7 subjects all of whom were either instructors or graduate students of psychology.

[2] H. Münsterberg: *Eternal Values,* 1909, chs. 9, 10.

[3] *Cf.* Ladd and Woodworth: *Physiological Psychology,* 1911, p. 597.

[4] McDougall: Mind, 1902, N. S. XI, 316.

[5] Schumann: Beiträge zur Analyse der Gesichtswahrnehmungen, *Zeitschrift für Psych.,* XXIII, p. 1.

[6] James: *Principles of Psychology,* I, 1890, p. 443.

[7] Titchener: *Psychology of Feeling and Attention,* 1908, p. 228.
Material of like character has been collected by various workers,

The method used in the present experiment depends largely for its effectiveness on just such possibility of a double interpretation of the figures concerned. A manifold is given from which the mind may select and interpret just as in the case of the equivocal figures. The manifold in this case consists of a number, usually six, small objects cut from cardboard. (Plate I, figs. 1-13.) These are arranged in various positions and combinations, and various possibilities of interpretation occur.

In regard to the relative merit of various figures such as the circle, the square, triangle, etc., some information has already been obtained. Fechner[8] finds that the degree of unit that joins the parts of a circle is higher than that which joins the parts of a straight line and that unity between the parts of an ellipse is higher than that of the circle. Puffer[9] discusses the forms found in use in the composition of pictures; pyramidal, diamond shape, diagonal, V shaped landscapes and square. She finds that the pyramidal is the commonest and the square less frequent. Gordon[10] speaks of the triangle as the simplest of enclosed forms, the square as less concentrated, and the circle as the symbol of completeness. Some of the results of the present investigation have a distinct bearing on this question, the triangle and circle proving to be the best forms to be felt as units, in tests of varying character.

Such results are important for Aesthetics, yet our main problem has to do with the question of unity alone. The question of the pleasure derived from unity does not enter in. The subjects in the experiment were instructed to give a judgment merely as to whether a unit was apperceived or not, and the feeling, pleasurable or otherwise, though noted at times, was in no way the object of study.

Thus the purpose at hand is to study the process involved in the act of the mind by which a number of elements are unified into a whole, to ascertain what conditions favor and what hinder the formation of units, to investigate the power of various factors involved and to test their relative importance. This brings us to a description of the experiment itself.

The material used consisted of small objects cut out of cardboard in various shapes (Plate I, fig. 1-13). These are reproduced slightly reduced (one sixth) from actual size used.

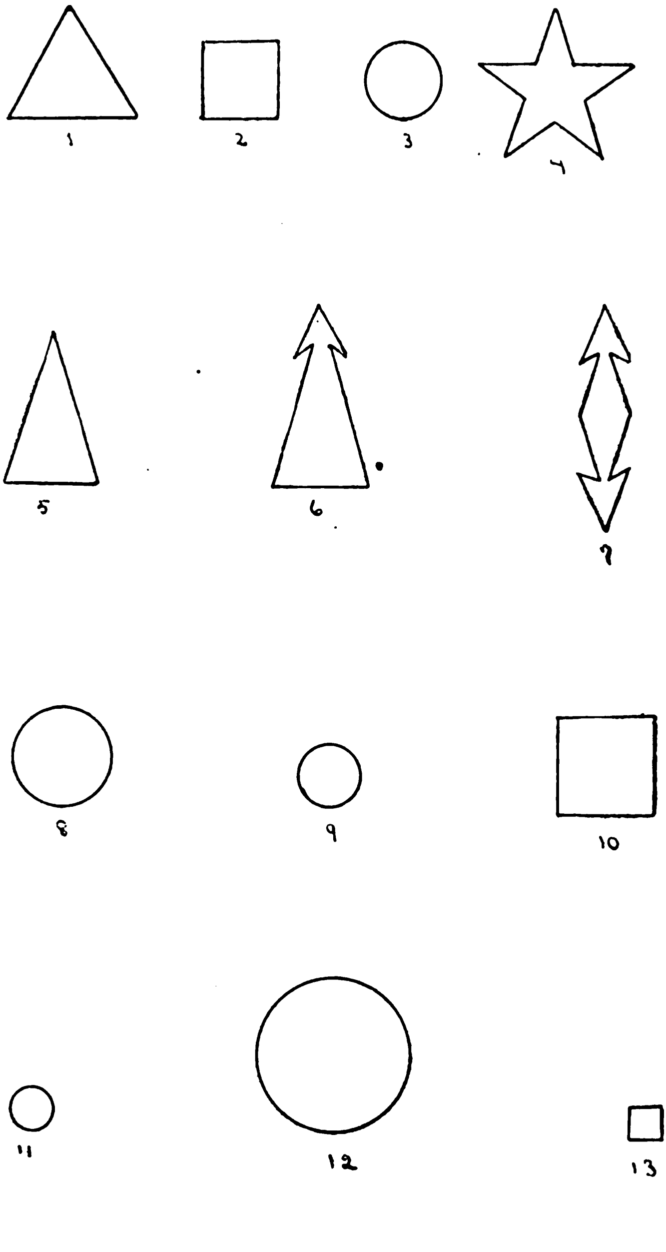
notably: Von Bezolt, The Theory of Color, p. 253, Wallace Wallin, Optical Illusions of Reversible Perspective, Mach. Analyse der Empfindungen, p. 164, Witmer, Analytical Psychology, p. 12 sq.

[8] G. T. Fechner: *Vorschule der Aesthetik*, 1876, p. 53.

[9] E. Puffer: *Psychology of Beauty*, 1905, p. 138 sq.

[10] K. Gordon: *Aesthetics*, 1909, p. 166 sq.

PLATE I.



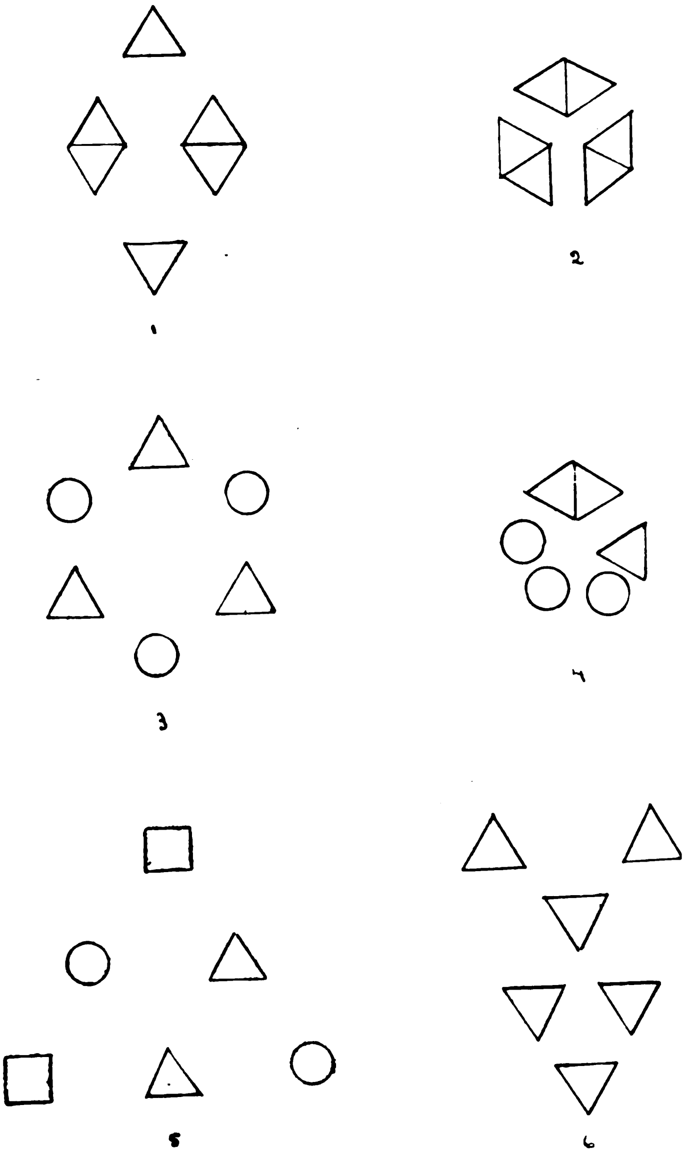
These objects, usually six in number, in some cases nine, were arranged on a black background in various figures within an area about 20 cm. square. (Cf. Plate II.) These figures were exposed by means of a tachistoscopic apparatus arranged as follows: The subject was seated before a black cardboard screen into which was fitted a diaphragm shutter with opening 3 cm. in diameter. On the other side of the screen was a cardboard surface also black, slightly inclined to the horizontal, and arranged to be in the field of view as seen by the subject, who had his eye at the opening of the shutter. The small pieces of cardboard were arranged on this surface. The figures were given an exposure of one-fifth to one-half second and the subject was asked to report in what way he apperceived the objects; whether they were united into a whole, or into subordinate groups, or whether they remained a manifold. The answer was recorded and the objects were rearranged. About thirty such exposures were given at one sitting, the arrangements of the objects being such as to throw light on some especial part of the problem. The objects used at first were white; later color was added, and in some of the final tests grey was used. The colors used were kept the same throughout, being red, green, blue, yellow, violet, orange, of the standard colors as given in the catalogue of Bradley. In one case only, two different shades of green, red, and violet were used. The time used was kept constant as far as possible. The shortest exposure was used at first for white objects on a black background. When objects of different shapes and colors were used, it was found necessary to give a longer exposure, and for the case where nine objects were used in relatively complicated figures a long exposure was needed. The influence of the time element, however, was considered only incidentally. In one case, only, the same figures were repeated using a longer time exposure. The object was to give sufficient time for the subject to be able to perceive the figure clearly, but not enough to allow him to study it, for it was desired to have merely the first impression recorded.

A unit as here used means not merely that the objects are felt as a whole simply because in the same field (the subjects were cautioned especially in this regard), but the individual parts must be felt as bound together, as belonging together in some way.

The factors investigated that have influence in the unit-making process are the following:

1. *Position.* The position or arrangement of the elements

Plate II.



may give the character of unity to the group, such as a symmetrical arrangement, arrangement in a geometrical form as a circle or a triangle, arrangement in lines, or it may be that mere contiguity may give unity.

2. *Form.* Like forms such as triangles or circles may be grouped together.

3. *Color.* Like colors may be grouped together to form a unit.

4. *Direction.* Like direction has the power to make a unit, and certainly the direction in which the parts point has at all times a powerful influence, even if it is not the predominant one.

5. *Size.* The size of the elements is found to be important in its influence on the grouping.

6. *Association.* The binding principle may be association with some idea already in the mind.

Some of the terms used in the course of the experiment need to be defined as to their exact application, and are as follows:

The term *unit* is used when reference is made to a whole formed of all the elements present in the field. The term *figure* is used when reference is made to the arrangement of the elements used. The term *group* is used when some of the elements form the whole, the others being neglected or going to form another group. Thus six objects might form three groups composed of two each. The term *manifold* is used when the objects in the field are seen as individuals and are not grouped.

Groups may be formed determined by various factors mentioned above. Thus *color group* refers to a group determined by color. *Form group* refers to a group determined by the form of the component parts. A *position group* is determined by the arrangement of its elements. Similarly the terms *direction group* and *size group* are used when direction and size respectively are the predominant factors that determine the group.

It often occurs that the subject can report more than one experience upon looking at the figure exposed, that is, he may see it in one way first, and then another interpretation will flash upon his consciousness. A unit may be his first experience, and then the figure will appear to be split into groups. All such experiences have been recorded. In calculating the results and making up the tables the numbers refer to the first experiences, yet it often happens that a consideration of the

second experience is helpful. In some cases certain ways of interpreting the figures will occur only as second experiences. If a unit is reported as first experience by four subjects and as second by three, it would show that the unity of the figure was felt by all, though in a less degree by some.

Form. The first experiment consisted in using six equilateral triangles, white on a black background, arranged in various figures. The objects at first were all the same shape. The next step was to study the effect of using objects of two different shapes, three triangles and three circles; and later a greater diversity of parts was used, two triangles, two circles and two squares. See Plates I and II. The figures in which the objects were arranged were, first, a circle, then a triangle, a diamond, a rectangle, a figure called for convenience a divided hexagon, and a figure something like an arrowhead. Each figure was repeated five times, each time with a different combination of its elements, the first exposure showing all like parts, the second two different shapes, the third greater diversity in the elements and so on to the fifth exposure. The results are tabulated in Table I. The exposures are numbered one to five. The numbers under the words *unit, group,* and *manifold* show the number of times the figure was judged to be the unit, group or manifold respectively. The answers as tabulated show a decrease in unity when unlike forms are used as elements of the figure. The judgment would often be that the figure was a poor unit, so the distinction is made in the table between a good and a poor unit.

From these results it appears clearly that the effect of introducing different shapes as elements in the figures is to lessen the unity and at times to break it completely. The total number of cases of a good unity for all the figures when like forms are used is 28 out of a possible 42; that is, 66%, while in case of the same figure under like conditions except that unlike forms are used, the number of cases of good unity is only 18 out of a possible 168, or 10%.

We can also study in this series the comparative excellence as units of the figures used. Referring again to the table, we see that the triangle is a unit 32 times out of a possible 35, the circle 30 times, the diamond 21 times, the divided hexagon 16, while the arrowhead is a unit only 14 times, and the rectangle only 9 times. Of these figures with this time of exposure we can say that the triangle is the best unit, the circle next, both possessing a high degree of unity, the diamond comes not far behind, while the divided hexagon proves to be not so

good a unit because of the tendency to apperceive it as three groups. The arrowhead figure proves a poor unit, and the poorest unit is the rectangle.

TABLE I

Exposure	*Circle*				Exposure	*Triangle*			
	Unit Good	Unit Poor	Groups	Mani-fold		Unit Good	Unit Poor	Groups	Mani-fold
1	7				1	6	1		
2	2	5			2		7		
3	2	3		2	3		5	2	
4	1	5		1	4		7		
5	1	4		2	5		6		1
	30		5			32		3	
	Diamond					*Rectangle*			
1	6	1			1	2		2	3
2		3	4		2	1	1		5
3	1	2	4		3	1	1	5	
4		4	3		4	2		1	4
5		4	3		5	1		1	5
	21		14			9		26	
	Divided Hexagon					*Arrowhead*			
1	6		1		1	1	3		3
2	3	3	1		2	1	3	1	2
3	1	1	1	4	3		2		5
4		2	5		4	1	2		4
5			6	1	5		1		6
	16		19			14		21	

Color.[9] For the purpose of studying the effect of color six square objects were used of three different colors, two of each, in various combinations. These squares were arranged

to form the following figures: circle, cross, rectangle, a pyramid-shaped figure, triangle with apex below, and a monument-like shape with the parts contiguous. All possible combinations of the three colors were used. In general, color is found to have a decided influence on unity, but some figures keep their unity better than others. The comparative excellence of the above figures was found to be as follows, as is seen from Table II. The circle and cross are the best units, the monument next, the rectangle next, then the inverted triangle, and the pyramid-shaped figure the least unified.

TABLE II

	Unit	Manifold
Circle	27	6
Cross	27	6
Rectangle	17	15
Pyramid	8	24
Inverted triangle	15	16
Monument	24	7

Combinations of two colors were next tried. The same figures were used, formed of six squares as in the preceding experiment. The general result was that where colors were widely contrasting the figure would break up into two color groups. The subjects vary very much individually as to the influence of color upon their appreciation of form. The same general result as in the preceding experiment in regard to the unity of the different figures was found to hold. Only three cases of manifold were reported. The choice was usually be-

TABLE III

	Unit	Group	Manifold
Circle	16	18	1
Cross	21	14	
Rectangle	15	18	2
Pyramid	9	26	
Inverted triangle	18	17	
Monument	24	11	

tween a unit and two color groups. That there should be eleven cases of color grouping in the case of the monument where the parts were contiguous is significant for the effect of color. See Table III.

Color Opposed to Form. In the next experiment three squares and three triangles were used and combinations of two colors, the division according to color and according to form not coinciding. The object was to see whether color or form was the stronger factor in determining a group. With the time used, one-fifth second, the result proved to be that color without question attracted the attention first, and judgments were uniformly given in favor of color groups. A longer time, one-half second, was given, and the same series of figures was used again without difference in the results as to appreciation of distinction in form. Some subjects reported that they did not notice that there were different shapes used, the impression being merely spots of color.

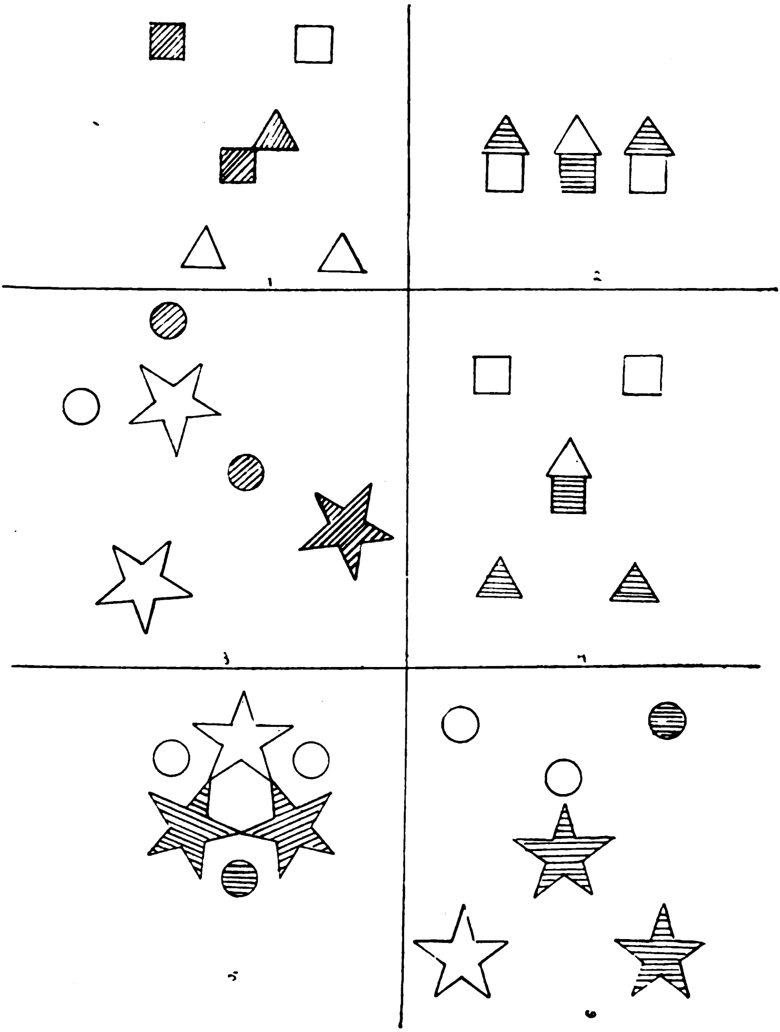
The effect of the longer time was to increase the number of units and color groups, and to decrease the number of cases of a manifold. In the first series, time one-fifth second, there were reported 51 units, 70 color groups, 16 manifold; with longer time, one-half second, there were reported 60 units, 85 color groups, 9 manifold.

As to the relative merit of the triangle and the circle the triangle was judged a unit slightly more often than the circle. In the first series the circle was reported a unit 24 times and the triangle 27 times; with the longer exposure the circle was a unit 29 times and the triangle 31 times. Contiguity proved a stronger factor than color when the arrangement was in three groups of two each. In the first series contiguity formed the group 28 times, color 10 times; with longer time, contiguity formed the group 25 times, color 13 times.

With the forms used in the preceding experiment it was found that even with long exposure the influence of color was stronger than the influence of similarity of shape. The difference between square and triangle was evidently not sufficiently great to have much effect in forming groups. In order to test the matter further a greater difference in form was sought and large stars were now combined with circles of smaller size. See Plate III.

Five figures were used: an irregular figure, called a butterfly by some of the subjects (Plate III, fig. 3), the circle, the cross, an hour-glass shape, and another irregular arrangement. Six different combinations of colors were used of varying contrast-

PLATE III.



ing power, and the arrangement of colors within the figures were varied. The combinations used were blue-yellow, violet-green, orange-red, green-yellow, red-yellow, and red-green. Time used was one-half second. For the result see Table IV.

TABLE IV.

	Unit	Form-group	Color-group	Manifold
Butterfly	3	10	27	2
Cross	22	3	17	
Circle	25	3	14	
Hourglass	14	3	22	3
Irregular		9	28	5
Total	64	28	108	10

The total number of form groups to the total of color groups is 28 to 108, which amounts to 80% of the judgments in favor of color. A decided preference thus is shown for grouping by color, notwithstanding the fact that the difference in form was made very marked.

By referring to Table IV we see that the circle is the best unit of the figures chosen, it being a unit 25 times out of a possible 42, the cross very nearly as good, a unit 22 times, the hour glass 14 times, while the irregular arrangements show no unity except that the first figure is a unit three times through association with a butterfly.

The Effect of Suggestion. A test of the relative strength of the influence of color and form of a different nature from the preceding is now introduced. Hitherto the subjects have been requested to keep their minds as free as possible from any preconceived ideas, and to maintain the attitude of passive attention throughout. At this point the influence of suggestion was made a feature of the experiment. The same material was used as heretofore; squares and triangles being combined, and stars and circles. (Plate III, figs. 3, 4, 5, 6.) Fifteen figures were arranged in which form groups and color groups were opposed. Color combinations used were the same. red-green, red-orange, red-yellow, green-violet, green-yellow, blue-yellow. The time used was one-half second. Before each exposure the subject was instructed that he would see

the figure in a particular way. At one time a form group was suggested and at another a color group, the whole number of each being the same. The form of the instruction was as follows: "You will now see two color groups, one red and one green," or "You will now see two form groups, a group of stars and a group of circles," and so on. The result of this experiment is given in Table V.

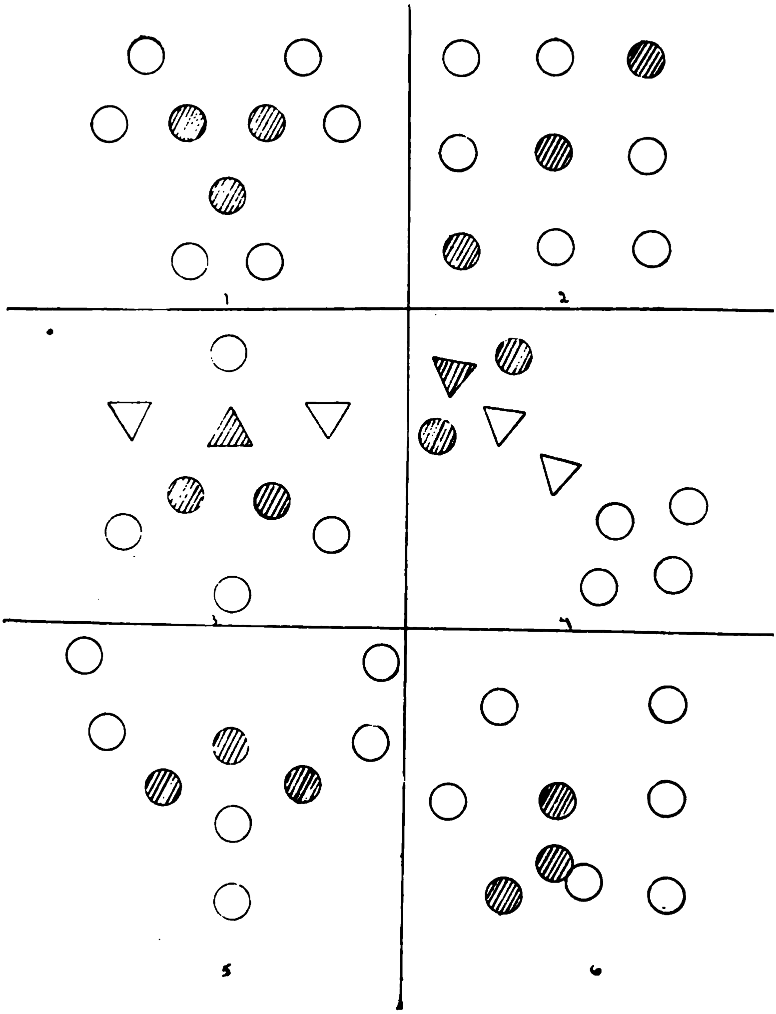
TABLE V.

	Unit	Form-group	Color-group	Manifold
Color-groups suggested. . .	6	10	89	
Form-groups suggested. . .	8	40	56	1

By studying the above table we find that when color is suggested there occur 85% of color-groups; when form is suggested the color-groups are still in the predominance, being 52% of the whole number of exposures; while when color is suggested, only 10% of the figures are seen as form-groups, and when form is suggested the number is increased only to 38%. This evidence seems quite conclusive in proving that color is a stronger unifying factor than similarity of form. A study of special figures (Plate III, figs. 4, 5) shows that some give color-groups preferably even when form-groups are suggested, while there are none to which a preference for form-groups is shown when color is suggested.

Form Opposed to Color with Increased Complexity of Material Used. A complication of the experiment was introduced by taking nine objects, variously combined into six different figures. See Plate IV. These figures were repeated on three different occasions. When presented the first time the objects were all of the same shape, circles. The next time two different forms were used, six circles and three triangles. The third time the objects used were four stars, three triangles, and two circles. On each occasion two colors were used, the division by color and the division by form not coinciding. The first figure (Plate IV, fig. 1) easily falls into three triangles, and for that reason is not so good a unit. The arrangement in a square figure 2 proves to be the best for

PLATE IV.



holding so many parts together. The circle with inner filling, figure 3, proves to be not so good a unit as the circle unfilled, the increased complexity of the figure decreases its unified character, showing that simplicity is an essential for a good unit. In case of figure 4, though the axis of the balance is diagonal, yet it is one of the best units of the series. The holding power of the diagonal line is strong. Figure 6 is a combination of three distinct position groups. These have no connection with each other and thus the figure does not make a good unit. In the short exposure the arrangement of the three groups is hardly detected, and the judgment of a unit is given only where there is some association, as of a letter or sign. Figure 5 is one of the best units of the series, second only to the square, association with a Y helping the unity, the symmetrical arrangement also assisting. The number of judgments in favor of unity for the six figures respectively, throughout the three series of exposures, was: figure 1, 21; figure 2, 60; figure 3, 43; figure 4, 48; figure 5, 53; figure 6, 10. Therefore, arranged in order of excellence as units, we have figures 2, 5, 4, 3, 1, 6.

The effect of introducing different forms into the figures in the second and third series of exposures was to decrease the number of color groups, and to increase the number of cases of manifold. Units remain about the same notwithstanding the fact that the repetition of the figures in this way increases the chance of their being seen as a unit on account of their familiarity. The number of color groups in Series I, where circles alone were used, was 131; in Series II, where circles and triangles were used, it was 87; in Series III, where stars, circles, and triangles were used, it was 36. The number of form groups, with circles and triangles used, was 10; the number of form groups, with stars, circles, and triangles used, was 12. Thus, though the effect of introducing different forms into the figures is to decrease the number of color groups, still the total number of color groups exceeds the number of form groups. This is a confirmation of the fact already established that color is a more important factor than form in determining groups.

Direction. The next factor that makes for unity to be studied is the influence of the direction in which the component parts point. For this purpose six isosceles triangles were chosen with acute angle at apex (Plate I, fig. 5), all of the same color, gray. These were arranged in various fig[illegible] triangle, rectangle, [illegible] and V inverted, triangle invert[illegible]

diamond. (Plate V.) At first, these triangles are made all to point upward, then downward, then to the right, then part up and part to the right, and finally part to the right and part to the left. This change of direction proves to have a very noticeable effect on the unity of the figure. The subjects were asked to discriminate between three degrees of unity; excellent, good and poor. The figures suffer a marked decrease in unity, according as the direction of the parts is changed. The results obtained by judgments on figure 1, Plate V, are given in Table VI. The other figures suffer a decrease in unity in like manner. The triangle with parts all pointing up (Plate V, fig. 1) is the best unit of all, all the subjects judging it an excellent unit.

TABLE VI.

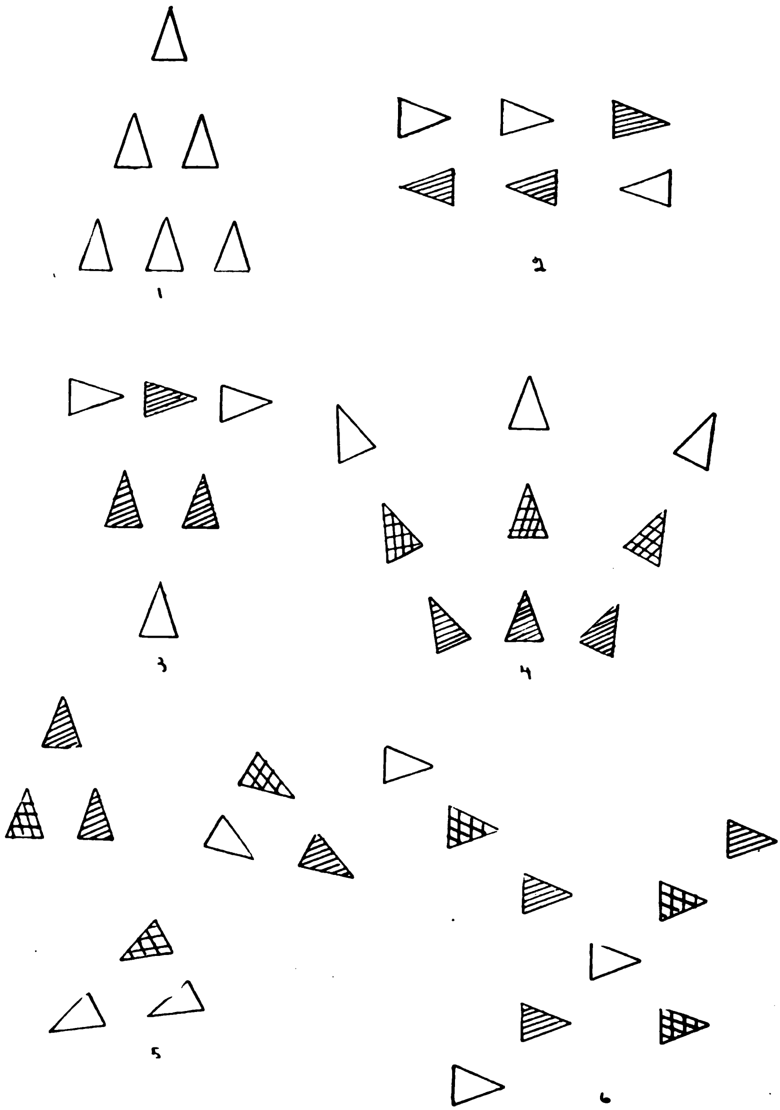
Direction of parts	Unit Excellent	Good	Poor	Groups	Manifold
Up	7				
Down	3	1	2	1	
To right	1	2	2	2	
3 up and 3 to right		2	2	3	
3 to right 3 to left		2	3	1	1

Color Opposed to Direction. The preceding experiment was repeated with the addition of color. The combinations of colors used were red-green, orange-violet, blue-yellow, and red-yellow, the color being evenly divided among the objects. Color in this case is the more potent influence in determining groups, the number of color groups to the number of direction groups being 57 to 13. See Table VII.

Another experiment opposing color to direction was made, using more complicated figures, nine objects and three colors. (Plate V, figs. 4, 5, 6.) The color combinations were violet-red-green, blue-yellow-red, orange-green-blue, red-yellow-violet. The number of cases of manifold were noticeably increased, confusion being caused by the change in direction of parts and the number of the colors used. The number of color groups slightly exceeded the number of direction groups, 39 to 23.

To emphasize direction another experiment was made using as material triangles tipped with an arrowhead, and diamond-

Plate V.



shaped objects tipped with arrowheads at both ends. (Plate I, figs. 6 and 7.) Six of these objects were used, three single and three double-direction, and two colors. The color combinations were red-green, orange-violet, blue-yellow, and red-yellow. Fifteen figures were used, and were then repeated with a different arrangement of the colors. Direction thus emphasized produced more groups than color, direction groups to color groups being 100 to 68. Fewer cases of unity and more cases of a manifold occur, inasmuch as both the distraction of color and of diversity of direction were present.

In general, color and direction were found to be fairly evenly balanced factors. Groups could be arranged to favor one or the other. Much depends on position and the arrangement of the colors. Color favored by position will prevail. Likewise when direction is assisted by position, direction prevails over color, as in the case of the arrangement in lines (Plate V, fig. 4), direction groups to color groups being four to two.

TABLE VII.

	Unit			Groups		Manifold
	Good	Medium	Poor	Direction	Color	
Six triangles, two colors...	28	45	44	13	57	22
Nine triangles, three colors..	15	42	33	23	39	59
Six arrowheads two colors...		2	8	100	68	22

Size. The relative size of the objects forming a group has heretofore not been regarded, all the objects in the field of vision being the same size or nearly so. The effect of size was now made a subject for investigation in itself. For this purpose circles were chosen to prevent any influence of direction inasmuch as the circle does not guide the eye beyond itself. Three large circles and three small ones were taken, all of uniform color, gray; this being to avoid the distraction of color. The size of the circles was 17 mm. and 6 mm. in diameter, respectively. (Plate I, figs. 8 and 11.) Time used was one-fifth second. Thirty different figures were presented, some symmetrical, some lines, some triangles, and some irregular. The result was more uniform than was the case with any other factor. The subjects seemed to be affected by size

in the same way and to the same extent. The three large circles were grouped together quite regularly and the three small circles also, even in cases where position would favor another grouping. Moreover, the group of large objects usually appeared first in order of time. The total of size groups perceived out of 210 exposures was 183, units 15, position groups 6, and manifold 6; that is, 87% of all ways of apperceiving the figures exposed consisted of groups according to size. Of these there were 49 cases of perceiving one group only, in which case the group was the one composed of the large circles. In 41 cases the large group was reported as being seen first and in 15 cases it was pronounced a better group or more prominent. Only in two cases was the group of small circles pronounced better, and in each of these the central position favored it.

A second experiment with size was tried in which nine circles were used, three sizes, 25 mm., 12 mm. and 6 mm. in diameter respectively. (Plate I, figs. 12, 3, 11.) The time used was longer, one-half second. The result corroborated the result of the preceding experiment. Groups were formed according to the size of the objects, 201 cases out of a possible 210, or 90%; only 5 cases of unity were reported, 3 position groups and one manifold. There were reported 87 cases of one group only, this being the group of the largest circles; 55 cases of two groups, consisting of the large and medium sized. Quite regularly there was a difference in time of apperception of the groups, the group of large circles being seen first, medium sized next, and smallest last. When two groups were apperceived, the large ones appeared first 44 times. The order of large, medium and small was reported 60 times, and the order of large, small, medium 8 times. In the latter case the group of small circles was usually in the center or some prominent position. The order medium, large, small was recorded once.

Color Opposed to Size. The material, figures and time were the same as for the first experiment with size, with the addition of color. The combinations of color were: red-green, blue-yellow, orange-violet, red-yellow, orange-green. There was a decided tendency for the formation of size groups rather than color groups. Of 190 exposures there were 147 cases of size groups or 81%, 9 cases of unity, 3 of position groups and 21 color groups. One group only, a group of large circles, was seen 46 times; and again, when both groups were seen, the group of large circles entered consciousness first. This

was reported 56 times, while the opposite order, small first and large next, was reported once in the case where the small circles outlined an outer triangle. It might be suggested that, inasmuch as size was the new feature introduced into the experiment, that might be the reason for its proving so powerful a factor. This consideration was outweighed by the fact that the series where size was opposed to color followed upon two preceding series where color had not been used, thus making color the additional feature of the experiment; so that novelty in itself can hardly account for the preponderating influence of size. Also, inasmuch as the subjects had been giving judgments of color groups for some time, it might be argued that this, if established as a habit, would influence their judgments in this case. Whether a new feature introduced or an habitual manner of apperceiving would be the stronger influence can hardly be decided, but the results would hardly be so uniform for all the subjects in case any uncontrolled influence were at work. The subjects were more unanimous in their response to the size factor than to any other in the whole series of experiments. Their susceptibility to color influence was markedly not uniform, and seemed to be quite an individual matter.

Form Opposed to Size. The material consisted of the large and small circles 17 mm. and 6 mm. in diameter, respectively; large and small squares, sides 15 mm. and 5 mm. respectively. The color used was gray. (Plate I, figs. 8, 11, 10, 13.) These were arranged in various figures. Size groups still predominated, though the percentage, 75%, was not so large as in the previous experiments. The number of position groups was slightly increased, being 19 out of 180 exposures. Units seen numbered 15, there were two cases of manifold, while only nine cases of form groups were reported. Again we find cases of the group of large objects seen first 13 times; and when both a group of large and a group of small were seen, the order in time of large, then small, occurred 78 times; the opposite order, small first, was reported once. For the most part the subjects reported that the difference in form did not particularly attract their attention, although at times it acted as a disturbing factor. Yet in the case of two subjects the squares stood out prominently contrasted with the circles. And of the 37 cases of form groups reported (and of these only nine were first experiences), there were 22 cases of one group only seen, and that the group of squares. One case of circles only occurred. Twice the order occurred of squares first, and then circles, when both were perceived.

Combinations of Different Sizes. The question arises as to just how much of a difference in size there must be in order to produce a marked effect in forming units. The combination of circles 17 mm. and 6 mm. in diameter, respectively, was effective, the difference in diameter being 11 mm.; also that of circles 25 mm., 12 mm., and 6 mm. in diameter, respectively, with difference of 13 mm. and 6 mm. The step was next taken to make the difference in size less in order to test the extent of its importance. Circles 17 mm., 12 mm. and 9 mm. in diameter respectively were used in three combinations: circles 12 mm. and 9 mm. in diameter, with the difference of 3 mm.; circles 17 mm. and 12 mm. in diameter, with the difference of 5 mm., and circles 17 mm. and 9 mm. in diameter, with difference 8 mm. Six objects, the same time, the same figures and the same combinations of colors were used as in the case of the preceding experiment where color was opposed to size. Only four subjects were available for this experiment, but the result was the same for all of them. The first combination of circles 12 mm. and 9 mm. in diameter, with difference 3 mm., produced no size groups, the subjects reporting that the difference in size was not noticeable, and accordingly color groups predominated. When the combination of circles 17 mm. and 12 mm. in diameter was used, the subjects reported that they noticed the difference in size, but it was not great enough, evidently, to act as a unifying factor, for here again no size groups occurred, and color groups predominated. But when the combination of circles 17 mm. and 9 mm. in diameter was used, with difference of 8 mm., size groups again appeared. The smallest difference in size, when size was effective, three sizes being used, was 6 mm. A difference of 5 mm. in diameter was ineffective, while a difference of 8 mm. produced size groups. The conclusion reached is that the difference in size, under the conditions of this test, must be at least 6 mm., and preferably 8 mm. in order that size might be effective as a unifying factor. Where the difference in diameter was 11 mm. there was no question of the effect of the element of size.

Summary. I. Position:[11] Contiguity makes for unity.

[11] Stratton considers that the interest in space form is more primitive than the sense of color. By form he means what we have here called position. He speaks of the "rivalry between the sensuous and the interconnective element," and shows that one or the other is subordinated. "If the attention is occupied with the color, the form and significance are in the background."

Stratton: Experimental Psychology and Culture, pp. 250 and 254

Position assists form to overcome color. Position in a line has great power. Position throws the balance in favor of color or direction. Position as an opposing factor to size forms a group. Of the geometrical figures used the triangle and circle are the best units, the diamond and square next. lines are effective, the rectangle is good and other figures in varying degrees. This estimation is merely tentative. It seems to be justified by the tests that have so far been made. It might be that other conditions would bring out different results.

II. Form: Differing forms used within the figure disturb the unity. The effect also is to interfere with color grouping, and to produce a manifold. Similarity of form may determine a group, but this is, perhaps, the weakest of the factors studied, and it proves to be weaker than color. The influence of form was found to be also weaker than size.

III. Color: Color proves to be a strong distracting influence. Color is a stronger influence than form in determining groups. It is stronger than form even when suggestion is used to assist. Color and direction are nearly equal factors. Color is weaker than size as a unit making factor.

IV. Direction: Direction is a distracting influence and is able to break a unit. The power of direction is nearly equal to that of color, and when assisted by position may be stronger than color.

V. Size: Size is the strongest factor studied, when the difference in the size of the objects is made sufficiently great. Size is stronger than position, color, and form.

Analysis of the Process of Unification. In studying the influence of these various factors in the course of the experiment, it has been possible to observe the process of unifying. This process is not the same for all. · Some minds are analytic while others may be called synthetic. The subjects have shown marked individual characteristics in their method of apperceiving. For instance, one subject will serve as an example of a pronounced synthetic type of mind. He shows the tendency to see the objects as individuals at first and to report a unit as a second experience. When questioned as to this habit, he said that the parts would persist in coming first, and the unit developed later. On the other hand, another subject illustrates the opposite tendency. A unit for him was the first experience in the majority of cases. The other subjects, while not as marked in type, exhibited many individual traits. Especially their susceptibility to color varied, three being ex-

tremely sensitive to the color influence, others not so much so, and one noticeably not influenced by it. This bears out the remarks of Stratton[12] in regard to the personal equation in the matter of form and color.

The processes of analysis and synthesis noticeable in the mental operations of the above-mentioned subjects may be found, the one or the other, in almost every judgment given throughout the experiment. It has been interesting to watch the influence of the various factors at work. We might say, in general, that the process in question is either destructive or constructive. A unit is seen and then is torn in pieces, or a manifold is seen and of the elements a unit may be constructed. Often the closing shutter would interrupt the process so that it was caught, as it were, half way, and the subject would report that he saw a unit, but that it was just about to break. A "tendency to break" was a frequent comment made when the force of the factor that was being studied, color, form, as the case might be, was not quite strong enough to prevent the unit and yet its influence was evident in consciousness. It seemed almost possible to measure the destructive tendency.

In studying the influence of the various factors at work, we find the following results: When two unit making factors are opposed to each other, the result varies according to the strength of the opposing elements, and it is possible to distinguish six different degrees of the influence of what may be termed the secondary factor. For one influence is usually the stronger and may be termed the primary or predominant factor, while the other is weaker and secondary.

1. The primary factor determines the group, while the secondary, though objectively present, does not enter the threshold of consciousness. This case occurs repeatedly. Many times when color and form were opposed, the subject would not be conscious at all that different shapes were used. Also when color and size were opposed, the difference in size being purposely made very slight, the difference of 3 mm. in the diameter of the circles was not noticed, and the subjects reported that the circles seemed about the same size.

2. The primary factor determines the group, while the secondary is just noticeable but is ineffective. In cases of unity determined by arrangement of the parts in a circle, colors used were apparent to the subject, but had no effect in disturbing the unity. Also where color was opposed to size, a difference in size of 5 mm. in the diameters of the circles was

[12] Stratton: l. c., p. 251.

noticed, but did not have power enough to influence the grouping.

3. The primary factor determines the group while the secondary is not only noticeable but has a decided effect. It may have a destructive tendency. Its effect is to act as a disturbing element, though it has not power enough to break the unit. Many cases occur where color is reported as a disturbing element. Direction was felt to be disturbing many times, for instance, in the case of Plate V, figure 3, a medium unit was reported, but the effect of motion due to the direction was felt.

4. The effect of the secondary factor is constructive. The primary factor determines the group while the secondary draws into it an additional element. There are many cases of this constructive influence, for example, the case where large circles form the group while a small red circle was drawn in through the influence of color. The very act of including the additional element was seen in process in one case. The large circles made the group while a tendency to include a small orange circle was felt, but the process was not completed. Three circles made a group determined by form, while a large square was added because of its size.

5. The factors may be evenly balanced in power and as a result cases of flüctuations or rivalry occur. In these cases of rivalry one or the other of the forces may win out and determine which group is to be apperceived; or a confusion arises, neither winning, and a manifold is then reported. Cases of rivalry between form and color occur, as in the case of Plate III, figure 3, and many others. Sometimes color wins out, sometimes the fluctuation persists and sometimes confusion results. There are cases of rivalry between direction and color, as Plate V, figs. 4, 6. In the case of Plate V, fig. 6, the conflict results in a manifold. In one case the process of grouping by color was actually interrupted by a sudden perception of direction groups. A size group and a position group were seen, while the rival tendencies of size and color produced a disagreeable feeling tone.

6. The primary factor determines the group when the power of the secondary factor suddenly increases, acts destructively, and causes confusion and a manifold results. A number of cases occurred where a unit was reported as a first experience and a manifold as a second. This shows just such an operation as described. The influence of color is often found to act in this way. A poor unit was reported as a first experience, then two groups as a second, and finally the disintegra-

tion was completed and a manifold was the result. The feeling of confusion often accompanied such an experience.

The data upon which these results have been based are too few to permit final conclusions upon all the points involved, but they have been presented for the suggestions they offer and should prove of value not only for theoretical aesthetics, but also for practical purposes especially in the fine arts, decorative art, and advertising.

THE INFLUENCE OF COLOR UPON MENTAL AND MOTOR EFFICIENCY*

By Sidney L. Pressey

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I. Problem

The problem of the study may be put as follows: Do different hues and brightnesses of general illumination have (*aside from* the obvious importance of these factors in conditioning the effective functioning of the visual apparatus) any specific influence upon mental or motor efficiency? The subject is obviously of great practical importance. If adequate lighting in factory or office tends to stimulate activity and increase the amount of work done, as well as to aid in visual efficiency, the matter is of no slight moment from a business point of view. If, as is often supposed, an office or

* From the Psychological Laboratory of Harvard University. The paper is a summary of the writer's thesis for the doctorate, which was presented in April, 1917, but which, because of pressure of other work, it has not been possible until now (October, 1919) to condense and arrange for publication.

school-room furnished in red has a distinctive effect, upon mood and upon sustained capacity for work, as compared with a room furnished in light green or buff, then the definite demonstration of that fact would be of decided practical value. The aim of the present study was to investigate experimentally, in the laboratory, this question as to the effect of brightness and of hue upon mental and physical work.

II. Previous Work Bearing Upon the Subject

As thus conceived, the study differed, in certain important particulars, from other investigations in this general field. In the first place, color combinations were not considered. In the second place, large surfaces, involving the subject's entire field of vision, were used, not small stimulus-areas (usually the subject worked in a room illuminated solely by a light of the desired brightness or hue). In the third place, the interest was in objective measurements of mental work and neuro-muscular tone, not in introspective reports. Finally, as has already been mentioned, the study was not concerned with the effects of hue or brightness upon visual acuity or ocular fatigue; any such effects were carefully avoided. Previous studies in the aesthetics of color have, however, most important bearings upon the writer's problem. A consideration of these previous studies may be conveniently taken up under three heads, according as the work was (*a*) introspective or observational, (*b*) experimental, or (*c*) analytical and critical.

(*a*) *Introspective and Observational Studies.* The earlier work treats rather of the artistic values of the different colors than of the narrower problem of the relation of color to affection. It is impossible in brief space to summarize the mass of this material. It may be said, shortly, that introspective consideration of the matter seemed to indicate that each color conditioned a specific emotional and affective response of great richness.

Goethe's "Farbenlehre" is the most important early treatment. There are "active" and "passive" colors. The hues from yellow through red are exciting, lively, arousing, the climax being reached in the red. From blue back to red is the minus or passive side; and at green is a balance of arousing and depressing influence which gives a feeling of peace and comfort. There is thus a complex and intimate relation between color and affection.[6] Such theories find their best known and most systematic formulation in Wundt.[40]

Anecdotal data with regard to color,[38] anthropological observations,[15] and studies in the history of art,[14] may also

contribute to the problem. Savages, and also many animals, show a marked fondness for bright colors and for shiny, brilliant objects. As evidences of the same tendency in civilized man may be cited the fondness for diamonds and other gems, for fire-works, for lacquer, gilding, boot-blacking, and the adoption of shiny metal for money.[15] Among the hues red has an outstanding position; the power of red to attract attention and arouse action, in both men and animals, is made much of.[10] However, there may be striking differences in the affective character of a hue, from one locality to another or one period to another. Thus yellow was in classical times a favorite color, and is now least liked.[11]

(*b*) *Experimental Studies.* Midway between such observational studies and the laboratory research comes a mass of more or less roughly controlled investigations, mostly with children, of which the monographs of Miss Shinn,[34] Mrs. Moore,[26] Preyer,[29] and Major,[24] and the papers of Winch,[39] Wells,[37] Jastrow,[18] may be mentioned. The gist of the results may be put in a sentence. There is an early fondness for bright colors, decreasing with age; an especially rapid dropping back of yellow, ending in least liking; in general blue and red are liked best.

The laboratory work may be summarized quite as briefly. There is an almost absurd difference of opinion among many of the experimenters as to the affective value of the various hues. And experimental studies of special problems in the aesthetics of color show little more (relevant to the present problem) than the extreme complexity of many reactions to color, and the multitude of factors which may play a part.

Cohn[7] states that his is the first experimental study of the aesthetics of color. He placed his subjects in a dark chamber, and used for stimuli small gelatine plates; the illumination was by daylight. He found, briefly, that saturated colors were preferred, but that as between different hues of equal saturation the choice was a matter of individual idiosyncrasy. On the whole, yellow was least liked; but the data are not considered adequate for any generalization in the matter. Cohn had seven subjects, and worked by the method of paired comparisons. Major[25] used colored papers, and a different method. He had four subjects. Neither antipathy for yellow nor preference for saturation was discovered. Cohn[8] thereupon experimented further, and confirmed his previous results as to saturation. Miss Baker[2] found the warm end of the spectrum most pleasant. Fernberger[13] found relative dislike for yellow and yellow-green. Miss Washburn[36] found that thirty-five college girls liked tints best, shades next, and saturated colors least. Blue was preferred, of the hues.

Some few papers have appeared which try to deal experimentally with the apperceptive elements in color attitudes. Thus Bullough[4] worked on the question of the apparent heaviness of colors. He concluded that a color looks light or heavy according to its luminosity,

stressing this element rather than any association of dark and low objects, or analogous explanations. Pierce[28] found that bright colors "weigh" more than dark ones in making up an ornamental balance; he explains this on the basis of the "action theory" and greater demand of the bright objects for eye-movements. Quantz[30] and Larguier des Bancels[20] agree that color affects the apparent size of objects. Thus red surfaces look larger than equal blue ones. But no satisfactory explanation is offered.

Investigation by the method of expression has led to similar contradictory results. Or rather, it may be said, that the best work of this type has given negative findings.

First to be mentioned of such studies should be Féré,[12] who, while experimenting with hysterics, found the most remarkable differential reactions to different tones, different tastes, different smells, and finally to different colors. If the patient was shown a red light there was an increase in pulse, a disturbance of breathing, a distinct increase in muscular strength as shown by the dynamometer. And, since he considered his neurotics to exihibit in marked degree what was true in kind of normal individuals, Féré concluded that, with normal individuals also, red had a dynamogenic effect.* A Leipzig study by Stefanescu-Goanga[35] comes to a somewhat similar conclusion. Striking results, giving confirmation of the Wundtian theory of the correlation of bodily changes and tri-dimensional feeling, were obtained. However, McDougall[23] found no specific effect of either hue or brightness upon reaction time.† Angell and Thompson[1] found no evidence of any specific association between organic changes and various visual stimuli. Shepherd's very careful work also failed to yield any evidence of such a connection.[33]

(*c*) *Analytical and Critical Studies.* The great importance of social influences, of literary and religious symbolism, and of language, in developing affective reactions to color is the one point constantly stressed by the more critical writers. That other than intrinsic values are important is apparent from the way color preferences change in art, and even with famous artists during their lifetimes. The effect of a color may be totally changed by slight change of circumstances. And such affective toning, though the result of previous associations, need by no means show any trace of those associations. The ideas and feelings have grown together, amalga-

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* Féré's work is extremely interesting. But surely these hysterics, who present as perhaps the fundamental symptom an extreme suggestibility, are exactly the persons who would respond most completely to the many subtle suggestions of language, art, and custom, which will shortly be mentioned (not to speak of possible unconscious suggestion from Féré himself). For the distinguishing of a true dynamogenesis, as distinct from the effect of such influences, no subjects could be less reliable.

† Prof. E. B. Holt has told the writer of a very similar study, made by Holt, with completely negative results. The average times, with the different hues, were almost perfectly identical.

mated their effects, until these are indistinguishable from innate hereditary attitudes.

So Müller-Freienfels[27] emphasizes the extent to which a color may be given an affective value simply by its name, as blood-red, or violet, or lilac, or orange, or lavender. The red-furnished room mentioned by Miss Calinich[6] is also of some interest. If the room was warm the reds seemed bright, lively, cheerful, warm and enlivening. But if the room was cold these same reds were dreary, depressing, with a peculiar, dead, chill effect.

The odd artistic career of yellow may also be instanced to show the extent to which even very special and artificial circumstances may have a profound effect upon the esteem with which a color is regarded. Yellow was a favorite color in classical times, and is now in the East. But there is, as has already been mentioned, some evidence to indicate that for the average European it is the color least liked. The change is (according to Havelock Ellis) due to the Church; the early church authorities looked upon the color with disfavor because of its association with pagan festivals and licentiousness, and succeeded in building up about the hue a group of symbolisms and associations of the most unpleasant character.[11]

Previous work bearing upon the writer's problem may, then, be briefly summarized as follows. (*a*) Introspective and observational studies show colors, as they appear in everyday experience, to have a marvelous richness and complexity of affective significance. There is the suggestion that color may be a conditioning factor of distinct importance in influencing the efficiency of mental work. (*b*) Laboratory experimentation for the most part fails to find any constant relationship between color and either introspective evaluation or organic reaction. (*c*) Analytical and critical studies emphasize the importance of artistic conventions, symbolism, language, and everyday association of certain colors to other sensations affectively toned, in giving an emotional connotation to the hues and brightnesses.

It would then seem reasonable to conclude that if color *does* have any fundamental physiological effect, such as would influence mental and motor efficiency, the connection must be of a very general and elementary nature; brightness may stimulate, or red irritate and distract, but more specific effects are hardly to be expected. It is some such relationship as this, between illumination and mental work, which is sought in the writer's experimentation.

III. Experimental Results

A. *Preliminary Experimental Definition of the Problem*

This experimentation began with a brief preliminary investigation, of an exploratory character, in which large sheets of colored paper were used. Nothing more need be said about

the method than that the subject was seated comfortably at a table, with a large sheet of colored paper arranged in front of him so as to be almost the only object within his field of vision. While in this situation, thus dominated visually by the color, the subject was asked to introspect, and to do certain tests. Nothing more was done with the tests than to make trial of them. But the introspection yielded some points of interest. In the first place, the finish and texture of the paper appeared exceedingly important, especially so far as pleasantness and hold on the attention were concerned; the "energizing" or "stimulating" effect seemed more dependent on the hue. Where there is a shimmer, or where the light falls across a rough finish so as to emphasize the roughnesses, the result is peculiarly disagreeable. Any smudges on the paper were sure to be noticed, and were likely to play a part in the association; thus the dominant feeling of one subject for an orange was uneasiness centering in the kinaesthesis of a desire to reach out and smooth an unevenness in the paper. In the second place, the introspection suggested that the special affective values often assigned to such colors as lavender, purple, orange might be due to other causes than the hue itself. Such colors are unusual, and so attract and hold the attention; because they are unusual, what associations they have are definite and concrete, and give great vividness. With repetition, however, these factors usually drop out, and the color is ranked along with the primaries, as more or less bright, stimulating, depressing; is sometimes liked merely because it has in it more or less yellow or blue. Finally, individual differences, both in the evaluation of the different colors and in the general type of reaction to them, were marked. In fact all of the types mentioned by Bullough[5] and Bradford,[3] with variations, appeared at one time or another, from crude sensory effect to complex personalizing attitude. The reports made excellent evidence for the complexity of responses to color mentioned in the preceding section.

Thus one subject reported of a striking reddish purple that he "liked the color. But there was no special effect, aside from the pleasantness; otherwise it was neutral." And at the end of the color period it was "about as before. No great fatigue. The affective value was somewhat less, but fairly durable." Very different are the descriptions of another man, who says of a rough-finished yellow that it is "heavy, like an overcoat; queer feeling; not pleasant, not unpleasant. Impression that color is deep, and thick. Kinaesthetic sensation in finger tips as result of thickness. Heaviness seems referred largely to texture." A light yellow green is "very pleasant, a buoyant feeling." Another day this same subject finds the same color "depressing; the color of grass when it's dying."

More explicitly associative is a report on yellow. "A slight greenish effect; brought up idea of grapefruit tree at the Exposition and emotions going with that trip. Now wonders how got green; had seen stripes of green." An orange was liked; but then, with a thought of the war, came an idea of this as the color of bloody water, and a feeling of repugnance.

Different still in type of response is the characterization of a red as "too aggressive; it pushes itself upon you, won't let you alone, rude. I don't like it."

Enough has already been said to suggest the unsatisfactoriness of colored papers as stimulus areas. In the first place, no adequate control of brightness was possible; but such control was clearly essential if an adequate analysis of the problem was to be made. In the second place, the texture of the papers, and any wrinkles or spots, played too important a part in the subject's reaction to make adequate control possible. And in the third place it was not feasible to arrange the experimental setting, with colored papers, as was necessary for the problem. The problem was to determine the effect of color, as a dominant but unobtrusive and natural element in a situation, on mental and physiological processes. If paper were to be used little less than to have the walls covered with it would suffice. Under the circumstances, trial was naturally made of colored lights.

With regard to the preliminary work with colored lights, nothing more need be said than that it was of the same general character as the systematic experimentation to be described shortly. Work with the colored lights was begun in March (1915) and the remainder of that school year was spent simply in trial of various tests, light screens, and so on. The results to be described in the next section were, therefore, obtained with methods which had been carefully elaborated in this trial series.

The preliminary work served chiefly to develop methods and setting and to make trial of tests. The introspective material may be summarized as follows. (1) Affective reaction to colors is highly variable, both from individual to individual, and from week to week with the same individual. No hue, except perhaps red, may be said to show characteristics of any constancy. (2) The reaction is very easily modified, or often wholly changed, by (*a*) subjective factors such as general condition, mood, constellation of ideas, or chance association, and by (*b*) incidental objective factors such as unevennesses in the colored surface, and especially the texture of this surface and the way in which the light is reflected from it (softly, or with a shimmer, glint, or glare). (3)

The reaction may be of all degrees of complexity, from simple sensory effect to elaborate associative development or subtle personalizing attitude.

Introspective treatment of the subject will be returned to, briefly, later.

B. *General Experimental Setting and Method*

The experimental setting developed during the last part of the preliminary work was continued, essentially unchanged, throughout the remainder of the study. It may be described as follows. One end of a long narrow dark room was shut off by a heavy black curtain, making a smaller room about 9x6 ft. Backed up against the curtain was a table three by five. Behind this, that is, facing the curtain and with his back to the end wall of the room, sat the subject. The table was covered with heavy unglazed drawing paper. Over the center of the table, and 26 in. above its surface, was the light-box (8 by 10 by 12 in.), held by a frame-work from the ceiling. This contained four 100 c. p. Mazdas. In the base of the box were slots, where the colored plates could be slipped in. The light box was dead black outside, but lined with metal to reflect the light downward; on the side of the box next to the subject was a black pasteboard drop hanging just low enough so that none of the direct light came into his face. At one end of the table, and dropping down almost to its surface, was the cord, with its glove finger, for the tapping test. Well off to the other side was the small block of wood with the two keys, for the reaction test; this was hooded over with a piece of the white drawing paper, to make it as unobtrusive as possible. The glove finger and the reaction key block were the only pieces of apparatus visible to the subject.

As has been said, none of the direct light came into the subject's face. The light shone directly, however, on almost the whole surface of the table, but very little beyond this. The situation was thus not at all uncomfortable for the subject. He sat easily at the table; the light came from a position not uncommon in ordinary lighting arrangements. There was no shimmer, glint, or glare, from the unglazed paper, only a soft, diffused, indirect illumination. And aside from this there was only the hardly-to-be-seen blackness of walls and curtain. This was the situation when the test lights were burning; when the "normal" light (a 16 c. p. Mazda fastened to the light-box on the side away from the subject) was on, between tests, the situation was very little changed. The lighting was still indirect, came from the same position approximately, and was of roughly the same brightness as the colors used.

On the other side of the curtain, and against the wall, was the experimenter's table. On this were the various recording devices, the theatre dimmer for equating the brightnesses of the colored lights, the time keys, and so on. Here also was the double-throw switch, arranged to throw on the test lights at the same instant that the "normal" was thrown off (and the reverse), so that there should be no period of darkness, or irregular interval, between the two stimuli. The experimenter's table was lit by a four candle-power lamp in a hooded "goose-neck" holder; it was wholly invisible to the subject behind his heavy curtain.

From now on the study consisted of two separate pieces of experimentation, making use, however, of the same tests, the same methods and the same setting; the "hue series" was planned to discover any possible effects of hue, independent of brightness, and the "brightness series" to isolate any influence of brightness, upon mental work. The plates used in the hue series were complementary blue, pure green, red. The stimulus lights were equated for brightness (by episcotister) in terms of the darkest, blue; to do this a theatre dimmer was used to shift the illuminating power of the lamps according to the absorption of the different plates.* The illumination, on the top of the table under the lamps, was about seven candle-meters. The brightness series with which most work was done was also a three-unit series. The dim light was obtained by stepping down the current with the dimmer and putting in the white screen; it gave an illumination on the surface of the table of about one candle-meter. The medium light was given by a single 32 c. p. Mazda, placed in among the larger lamps in the box so as to give the same distribution of light, and thrown in by double-throw switch after the "normal" in the same way. The bright light finally settled upon was the full power of the four 100 c. p. Mazdas.

For one semester, however, a five-hue and a four-brightness series were run. The five-hue series consisted of the hues above mentioned *plus* yellow and white. For white a special plate, a double thickness of "pot-opal" glass, was made; at the above-mentioned brightness the light through this was found to be almost pure physiological white. The four-brightness series consisted of the same two extremes as were used in the three-brightness series, but with the white of the hue series, and a medium bright light made by the white plate with the full power of the four Mazdas behind it, in place of the 32 c. p. "medium."

Three possible criticisms of this choice of stimuli remain for brief discussion. In the first place, it may be said that an effort should have been made to obtain monochromatic colors for the hue series. To

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* The writer is indebted to Dr. L. T. Troland for the equating of the brightnesses and for the rough analysis of the lights here given.

this it may be replied shortly that the problem of the study was essentially a problem in applied psychology, and that what was sought was the effect of colors such as might be used in actual schemes of illumination or decoration. The important requirement was that the lights should be seen by the subjects as red, green, and blue, of a not unusual composition or saturation, and that the saturations should be approximately equal. The lights were, as a matter of fact, almost invariably judged by the subject as simply blue, green, or red without admixture of other hues; and they were considered about equally saturated (the yellow was dropped partly because less saturated). Nothing more seemed essential. Monochromatic light, supposing it could have been obtained for use over such areas, would have been a highly artificial form of stimulus much less relevant to the practical problem.

It may be objected, in the second place, that more hues (and more brightnesses) should have been used. The difficulty was mainly a matter of method, and of experimental practicability. Most of the subjects came to the experiment only once a week, for a 50 min. period; five or six colors meant only seven or eight minutes with each. The problem, however, was to determine, not the effect of the presentation of the color, but the influence of its continued presence as part of a work situation. Longer periods were thus clearly necessary. So it seemed best, for this and various other reasons, to limit the experimentation to intensive study of three hues, and three brightnesses. Practically all the previous work (not to mention the writer's own preliminary study) indicated the colors at the two ends of the spectrum as the hues most diverse, fundamentally, in their effects. It seemed reasonable to suppose, therefore, that an intensive study of red, of green, and of blue would indicate with fair adequacy the direction and amount of the influence, if any were to be found, and give the best evidence for negative conclusions in case the results were negative.

In the third place, criticism might be made of the "very bright" light of the brightness series, as too strong, possibly so bright as to trouble the eyes, and so introduce factors not relevant to the problem. The writer expected such difficulties. And he was much surprised to find, after extended trial with all the subjects, that in most instances this bright light was the most pleasant of the series! The subject received only indirect, diffused light; there was no glint, glare, or other unpleasant distribution. Under these circumstances this light was usually introspected upon as stimulating, cheering, very pleasant. And anything less bright failed to give the desired distinctness from the "medium" light.

As has already been intimated, most of the work was done with three hues, or three brightnesses, to the hour. This permitted 12 to 15 min. with each color, with intermissions between colors of about 4 min. The colors were given in a definite routine; if, for instance, subject "A" was given on the first day red first, green next and blue last, then for his next hour (the following week) he was given green, blue, red, and the third hour blue, red, green. After three hours, then, each color had appeared once in each position in the hour. This made up a unit, within which the various factors of the work curve (fatigue, practice, and so on) were evenly

distributed among the colors. The next group of three would begin red, blue, green: in this way every possible order was tried. Between colors the "normal" light already mentioned illuminated the subject's room.

The number of subjects varied from one semester to another; usually there were six or eight, and two came twice a week, the rest once. Subjects who came twice a week were given one hour with the hue, and the other hour with the brightness series; the other subjects were divided up equally between the two series. Regular hours were assigned; the experimentation was done in the morning, in hour periods, between nine and one. The subjects were all trained observers, and familiar with laboratory methods and technique.

In the entire study (including the preliminary work) twenty-six persons at one time or another acted as subjects. One was a professor of psychology; seventeen were graduate students with experience in research. Eight were Harvard undergraduates doing advanced work in psychology; three of these were carrying on independent research. Three of the graduate students were women. It should, perhaps, be added that the study was made during the university years 1914-1915, 1915-1916, and 1916-1917 to the first of March.

Much difficulty was found in securing satisfactory tests, because of the fact that the purpose of the experiment prevented the employment of any test requiring use of the eyes; the purpose was not to study visual acuity with lights of different colors, or the effects of eye strain in dim light on mental work, but to discover whether *apart from such factors* different hues and different brightness had any influence on general neural tone and mental efficiency. The vast majority of tests previously used in such comparative studies (studies of the effects of drugs, for instance) were thus barred from the writer's experimentation. And much time was spent in simply developing tests which would meet these special requirements. The tests finally adopted will be described in detail later, along with the presentation of the results obtained. A large number of other tests (as dynamometer measurements, maximum speed in tapping, several forms of memory test) were tried, but found unsatisfactory and rejected.

The test routine varied, of couse, with the tests employed. The first of the work was done with the following tests, and in the following order. First, under the normal light, the subject tapped for 30 sec. (this was not the usual tapping test, but a tapping "at most comfortable rate," as will be described later). Then the normal light was thrown off and the test color on; the tapping, however, continued for another 45 sec. Introspection was now called for. After this, the multiplication was given, then the pressure test, then the memorizing. Forty-five seconds more of tapping followed this. Finally, introspection

was asked as to any change in the influence of the color from that reported when the light first appeared. The normal light was now thrown in, and the subject rested for 4 min. while the experimenter changed the colored plates, adjusted the theatre dimmer, slipped new smoked papers on the kymograph drums and adjusted the recording apparatus. Then the same programme was repeated with the other two hues. Introspection was called for only during the first two or three weeks at the beginning and end of a half year's work; for the greater part of the time the work was wholly objective.

This general arrangement was adhered to throughout the experimentation. As sufficient data were accumulated with a test it was dropped, and a new test substituted; the routine, however, remained essentially the same. Work in the color series and in the brightness series was also throughout the same; there were the same conditions, the same tests, and the same routine; so that the two series were in all respects strictly comparable.

Every half year saw a change in programme of some sort. With a new test three hours of practice were allowed, as a rule, before results were used. Data from twelve hours following this were the aim. This was not always possible, however. By the methods used, the results with a given color, in a given hour, and on a given test, obtained their significance from comparison with the results, on that test, under *all* the other lights. If now, reaction times under green were lengthened by the sound of the bell at the end of the hour then all the results, with the other colors as well, were warped in their significance. So the rule was made that no results from any hour which was interrupted, in any way, on any test, should be included. A large amount of material was, of course, eliminated by this rule; but the eliminations seemed necessary, if the subtle effects sought were not to be buried completely under such chance variations. The endeavor was also made to obtain, in each series, results from at least six individuals, on each test. This was, again, not always possible (or worth while) but was done in the majority of cases.

A word remains to be said regarding the handling and interpretation of the data. In the investigation of any such subtle influences as were being sought chance variations in the results are certain to be large, and differences (if any) due to the factors studied, small. Satisfactory interpretation and evaluation of the data are thus extremely difficult. The situation is, essentially, too complex to make possible any summary of the consistency of the findings, which could be treated statistically. Instead, a judgment upon the reliability of the data has been attempted, based upon three related, but distinct, sets of facts: (1) the consistency of the results obtained from each subject. on each single test; (2) the degree of agreement among different subjects on each test; and (3) the consistency of the results from test to test with each subject (a correlative consideration here being, of course, the consistency of the group from test to test). Any final numerical statement of the reliability of the findings must, of necessity, be partial and inadequate. Instead, an attempt has been

made to bring all the facts, considered from these three points of view, together in a judgment as to the probable meaning of the data.

The writer is not at all sure, now (1919), that a more determinate and decisive statistical treatment of the material would not have been possible. But, in defence of the procedure adopted, it may be pointed out that practically all the well-known studies having similar statistical problems have been quite as indecisive in their statistical statement. Most similar, in the statistical problem presented, are studies of the effects of drugs, of which the monographs of Hollingworth[17] and Dodge[9] may be cited. And it might be pointed out in this connection that, so far as difficulties of control and analysis are concerned, study of the effect of only three colors, in one short hour, is not a little analogous to an attempt to study the effect of three drugs in that brief space of time. The problem was not an easy one, and abounded in unusual difficulties of analysis and statistical treatment.

The methods used will be discussed in more detail in considering the results obtained with each test.

C. *Results*

1. *The Effect of Color upon Rate of Rhythmic Movement of the Finger.* This test was planned to give an indication of any change in neuro-muscular tone and tendency to movement. It was, essentially, an ergograph with minimal resistance. Clamped to the end of the table was a rod which reached obliquely in over the table top. The end was about 15 in. above the table, and the same distance in from the table end. To the rod was fastened a pulley. Through this ran a silk cord, to which was sewed a glove finger. The subject put his left fore-finger in the glove, and rested his arm easily on a support which kept his finger free above the surface of the table. The test consisted simply in moving the finger up and down with a tapping movement. The white silk cord ran over to the experimenters' table, where it was fastened to a marker which wrote on a smoked drum, and to the resistance, a single piece of ordinary rubber band. The subject was told to tap at an easy, comfortable rate, which could be kept up without fatigue during the minute tapping period; to find the rate which was most natural and comfortable for him; then to forget all about the test, letting the movement go on automatically. There was a specific caution against any effort at speed.

The test should, the writer felt, be quite highly sensitive to any energizing or stimulating effect the colors might have. If red *did* excite, or a dim light subdue, this ought to show easily in such a half-conscious rhythmic activity as this simple movement, up and down, of the finger. But there were other advantages. Practice effects were unimportant; there was no learning; there should be nothing of those

wide variations likely to appear when there is any requirement of maximal effort.

The place of the test in the routine has already been described. The tapping was begun 30 sec. before the stimulus light was turned on, and was continued for 45 sec. after this. The other tests were then given. And finally, just before the stimulus light was turned off, there was tapping again for another 45 sec. Time was marked off in 15 sec. units. The results were handled in terms of these units. The number of taps in the first two quarter-minutes under the normal light were averaged to give the average normal for the period. The number of taps in each one of the six quarter-minutes under the stimulus light were then scored so much *plus* or *minus* from this normal for the period. And the difference between the variation from its normal produced by red, and the variation from *its* normal produced by the blue, gave the difference in effect of these two colors.

This final figure, it should be emphasized, was almost wholly free from the influence of constant errors. With some of the tests, systematic variation through the hour, due chiefly to difficulty in getting started with the work, and to fatigue in the last period, were considerable. However, with this arrangement of the tapping, the normal, for each period, just preceded the color period itself. If red came last in the hour on a given day, its normal came near the last too; the normal took account of the variations through the hour (as well as of the daily variations), and none of these irrelevant factors got itself into the results.

In handling the results the number of taps, in each 15 sec. under the color, was first scored as so much *plus* or *minus* from the average number of taps per 15 sec. under the normal light, as has been just described. The variations for the first three quarter-minutes under the color were then added, giving the total variation for the first 45 sec.; and the variations for the last three quarter-minutes also added. For instance, suppose the number of taps per 15 sec. ran as follows:

		Green		
	normal	first 45 sec.		last 45 sec.
	17 15	16 17 19	(other tests)	17 19 20
This would be scored				
	16	0 1 3 (4)		1 3 4 (8)

To obtain the final percentage statement, the average normal was multiplied by three, to give the average normal for 45 sec. and the succeeding variations per 45 sec. stated as per cents of this. The above record would thus become

		Green	
	normal	first 45 sec.	second 45 sec.
	48	4	8
and in per cents	48	8%	17%

This could, of course, have been done less elaborately by running a normal of 45 sec. instead of 30 sec. But since the work was begun with a shorter normal it was so continued, and the above method of handling used.

TABLE I

TAPPING RATES: IN TERMS OF % VARIATION FROM "NORMAL"

Four Brightness Series

Sub.	Av. Nor. 45′	I 1st 45′, av.	I 1st 45′, m.v.	I Last av.	I Last m.v.	II 1st 45′, av.	II 1st 45′, m.v.	II Last av.	II Last m.v.	III 1st 45′, av.	III 1st 45′, m.v.	III Last av.	III Last m.v.	IV 1st 45′, av.	IV 1st 45′, m.v.	IV Last av.	IV Last m.v.
And......	46	5	6	5	11	—2	4	0	10	—3	6	—1	11	2	10	—6	10
Dav......	55	—2	3	13	5	1	3	3	7	0	2	2	11	2	4	1	9
Fin.......	49	—8	6	—6	9	—12	11	—1	10	—5	3	—2	4	—12	6	—11	10
Kal.......	102	7	6	12	14	5	7	10	13	—2	7	3	20	—11	9	—5	20
Average	...	.5	5	6	10	—2	6	3	10	—2.5	4	.5	11	—5	7	—5	12

(Ten hours each subject.)

Three Brightness Series

Sub.	Av. Nor.	I 1st 45′ av.	I 1st 45′ m.v.	I Last av.	I Last m.v.	II 1st 45′ av.	II 1st 45′ mv.	II Last av.	II Last m.v.	III 1st 45′, av.	III 1st 45′, m.v.	III Last av.	III Last m.v.
And..................................	62	16	7	18	12	2	5	13	9	0	8	5	9
Cur..................................	68	4	5	9	6	6	3	2	8	2	3	11	12
Dav..................................	77	7	8	9	4	0	2	6	10	7	10	4	7
Woolb...............................	51	1	5	0	7	3	3	—4	8	0	3	—7	10
Mor..................................	124	6	7	12	8	11	10	16	11	7	5	18	13
Average............................	...	7	6	10	7	4	5	7	9	3	6	6	10

(For each subject in order the hours were 20, 10, 8, 9, 9.)

TABTE I—*Continued*

Five Hue Series

Subj.	Av. N.	Red av.	Red m.v.	Red av.	Red m.v.	Yellow av.	Yellow m.v.	Yellow av.	Yellow m.v	Green av.	Green m.v.	Green av.	Green m.v.	Blue av.	Blue m.v.	Blue av.	Blue m.v.	White av.	White m.v.	White av.	White m.v.
Br.	93	2	4	8	3	2	3	4	2	—2	3	0	6	2	2	0	3	—2	2	5	5
Ch.	84	—3	6	6	9	0	6	5	2	—2	3	2	5	1	6	2	7	0	2	10	4
Da.	57	0	5	9	8	0	5	1	7	2	2	12	6	0	4	1	8	—1	3	0	7
Ohl.	73	3	4	6	4	3	7	0	6	2	4	3	4	0	5	3	4	3	5	1	7
La.	109	—2	4	7	7	2	3	10	5	0	2	10	6	1	4	4	4	—1	3	4	6
Average	...	0	5	7	6	1	5	4	4	0	3	5	5	1	4	2	5	0	3	4	6

(Ten hours each subject.)

Three Hue Series

Subj.	Av. N.	Red av.	Red m.v.	Red av.	Red m.v.	Green av.	Green m.v.	Green av.	Green m.v.	Blue av.	Blue m.v.	Blue av.	Blue m.v.
Allp.	147	6	4	4	7	6	3	6	7	3	6	2	6
La.	107	2	4	6	6	1	3	4	6	1	2	9	9
Bul.	44	3	4	9	8	2	5	1	12	3	4	6	8
Br.	148	3	2	5	3	—2	2	—1	3	—1	3	3	3
Ch.	106	3	4	2	14	0	3	12	8	0	3	4	12
Wo.	49	—2	2	—6	11	1	3	1	9	0	4	1	7
Sp.	60	10	.	1	14	1.5	12	6	10	0	4	9	6
Average	...	4	3	3	9	1	4	4	8	1	4	5	7

(Twelve hours three subjects, nine hours four subjects.)

The m. v. after the group average is the average of the m. v.'s, not the m. v. of the averages. In handling the per cents, fractions of one per cent were read as the nearest whole number.

The results for the brightness series are given in the following table (Table I). They are stated, as has just been described, as per cent variation, from average under normal light for 45 sec., for 45 sec. under the color. The first series was run with four lights, bright, moderately bright, medium and dim. In this series were four subjects; the number of hours for each was ten. In the second series three brightnesses were used; the first and the last were the same as in the first series, but in place of the moderately bright and medium light a new medium, the 32 c. p. Mazda, was substituted. In this series were five subjects, two of whom had also been in the previous series. The number of hours with one subject was twenty, with one ten, with one eight, and with two nine.

The results may be summarized as follows. Of seven subjects five, including those from whom there are most results, show an increase in rate of tapping with bright light, and a decrease in dim light. One subject showed a slight, the other a more considerable, reversal of these results. In the case of this last subject, there is some evidence to suggest a special, somewhat morbid, peculiarity in this respect. For those subjects giving positive findings there was not only an increase under bright light, but a decrease under the dim light as compared with rate under the medium light. There was also no evidence of decrease of these positive findings through a semester; one subject, kept at the test for a year and a half with a view to obtaining evidence on this question, gave as positive results at the last as at the beginning.

It must be emphasized that the results from the two tapping periods, the first 45 sec. and the last 45 sec., have a very different significance as regards the possible effect of the lights. Any change in the first period, when the light had just been thrown on, might be due simply to a shift of attention, or some similar transitory element in the situation. The results from the last 45 sec. should be of much more importance, since the stimulus light has been on, by that time, some 12 min., and any effect then observed must be of some permanence.

As a matter of fact, the results are most distinctive in this last period. The group averages show greatest number of taps for the brightest light, and a regular and even decrease down to the least number for the dimmest light. When the groups are analyzed it appears, in the first place, that the two subjects who worked in both series agree substantially, in the indications they give under the two somewhat different conditions. In the first group of four (with four lights) all the subjects agree in showing most rapid tapping with the bright light, slowest with the dim. In the second group of five, three subjects give this same result. One, however, shows a slight, the other a fairly distinct increase with the dim light over the bright one. In this last case, introspection may throw some light on the situation. This subject (Mo [illegible] was extremely fond of dim lighting. This was

due, he said, to a general liking for subdued light effects. He was fond of sitting in his room in the dark, or with the lights low, was very fond of candle-light, could think best when the lighting was dim. With the other subject giving opposed results there is no such clear-cut preference, though he did say once or twice that he could think better in the dark.

The results with the hues may be very briefly summarized. No differential effect of different hues, at equal brightnesses, upon rate of tapping appears. Two series were run, one consisting of red, yellow, green, blue, white, the other of red, green, blue. Eight subjects took part, five in the first series, four of these and three more in the second. It may be said, shortly, that no consistent findings appear with regard to any one of the hues studied. (See Table I.)

2. *The Effect of Color upon (a) Pulse and Respiration, (b) Estimates of Pressure, and (c) Judgments of the Pleasantness of Touch Substances.* It may be said at once that the work with pulse and respiration gave results which were entirely negative. The work was very carefully done; records were taken both at the beginning and the end of the color period, and studied with reference to both rate and shape of curve. But nothing appeared which could be correlated, in any way, with either brightness or hue.

The experiment of Stefanescu-Goanga,[35] already mentioned, was also largely repeated, with the writer's setting, but with negative results.

These negative findings are not, of course, proof positive of lack of any effect of hue or brightness upon the physiological processes. There may be an effect, and of a cumulative importance, on respiration and pulse. But the changes under such conditions must be, within the time any convenient measurement might extend (30, 45, or even 60 sec.) so minute as to be probably below the threshold of even the best technique.

It may also be said, with equal brevity, that results with the "pressure" and "pleasantness" tests were similarly negative. No influence of either brightness or hue, upon performance with these two tests, could be discovered.

The apparatus for the "pressure" test was simply an ordinary postage scale. For 1 min., during the 5 min. intermission between colors, the subject practiced pressing down exactly 16 oz., while looking at the dial. The scale was then taken away, but presented again, when the time during the color period came for the test; the subject was now given five trials at pressing down 16 oz. as previously practiced, but this time with the dial turned away from him. The record taken was the amount actually pressed at each attempt (the amounts pressed in the five trials being summed to make the "pressure" score for each color).

Just what function the test might be supposed to sample was not clear. Perhaps a bright light, or a red, if stimulating, would bring

about a more energetic push on the scale, and an overestimate. And a dim light might bring about less activity. But the fact was that no such relationships appeared. The test was interesting, but of no value, at least for the writer's experiment.

The experimentation with the touch substances was almost as simple and direct. A large variety of touch substances (different kinds of cloth, wood, metal, and so on) were fastened to small blocks of wood (one and one-fourth by two in. and one-half in. thick). Twenty of these blocks were put in a shallow box just big enough to hold them, five in a row with four rows. This box was covered with a hood of the heavy, unglazed drawing paper. When the time came for this test, the box was put on the table in front of the subject who, with his right hand, beginning at the upper left-hand corner, worked across the top row to the right. Each substance was felt in turn, and its pleasantness expressed on a scale of seven, where one was most unpleasant, four indifferent, and seven most pleasant. After the top row was finished, the subject dropped his hand to the next row, working from right to left; and so on till the twenty substances were all judged. The hood hid the substances from view, so that the subject had only a touch acquaintance with the materials.

After the test was finished the box was taken from the table, and the other tests given. During the intermission between color periods the experimenter, by a simple arrangement, completely and systematically changed the order of the substances. And from week to week the substances used were changed. The purpose in each case was to break up any memory of previous judgments on a particular substance, to make each estimate a product simply of the two present factors, the touch substance as it felt now, and the present affective attitude caused by the color. The results were handled by simply adding the values assigned to the twenty substances under each color, giving a total which might, perhaps, be called the affective level under the given light. The test seemed to the writer to have possibilities. But the findings were altogether negative. He still feels, though, that something of this sort might yield interesting data in a study of some other problem, to which it was better adapted.

3. *The Effect of Color upon Rate of Multiplying.* No study of this type would be complete without some sort of arithmetical test. Such tests seemed almost impossible of control sufficient for the purposes of the present problem. Little help could be obtained from previous work, since practically all tests previously used were ruled out, from the start, by the requirement that there should be no visual presentation. Nevertheless, the writer felt that an adequate study of his problem required at least an attempt at measurement of possible effects of color upon some such relatively complex form of mental work.

The test, as finally made up, consisted of multiplications of one-place by two-place numbers. Eleven such multiplications were given with each color; a test sheet, for a given day, thus consisted of three rows each having eleven problems. Three such sheets were prepared, enough for three weeks (or

a single series, where the colors came each once first in the hour). The three were then repeated in order. But the order of the colors was varied, so that any difference in difficulty of the different groups of eleven was distributed. The record was in time for each multiplication, as taken with a stop-watch, and in errors. In handling the material, that multiplication of the eleven which had the longest time was struck out; quite frequently a time would run much beyond the average as a result of some slight distraction; an elimination of the longest time of each eleven allowed, to some extent, for these variations. The times for the remaining ten were summed.

TABLE II

TIME IN MULTIPLYING (BRIGHTNESS SERIES) IN TERMS OF % OF AVERAGE TIME FOR HOURS

Four Brightness Series

	I. (bright)		II.		III.		IV. (dim)	
Subject	av.	m.v.	av.	m.v.	av.	m.v.	av.	m.v.
And	89	13.0	101	13.0	106	13.0	103	8.0
Dav	96	5.6	106	9.5	101	7.8	97	9.8
Fin	97	3.5	99	7.5	104	9.0	99	8.0
Kal	99	9.0	94	11.0	97	5.0	109	6.0
Average	95	7.7	100	10.2	102	8.7	102	7.9

(Ten hours each subject.)

Three Brightness Series

	I.		II.		III.	
And	100	6.2	99	4.7	101	5.6
Cur	95	7.6	101	10.0	104	7.6
Dav	99	4.9	99	4.8	102	5.3
Woolb	97	7.0	102	3.9	100	6.3
Mor	100	9.5	100	9.3	100	10.1
Average	98	7.0	100	6.5	101	6.9

(For each subject in order the number of hours is 10, 10, 8, 9, 9.)

TIME IN MULTIPLYING (HUE SERIES). IN TERMS OF % OF AVERAGE TIME FOR HOUR

	Red		Green		Blue	
Subject	av.	m.v.	av.	m.v.	av.	m.v.
Allp	99	4.7	101	6.2	99	8.2
La	101	4.4	102	4.5	97	2.7
Bul	100	8.3	99	7.9	100	8.8
Br	103	6.4	98	5.6	99	3.6
Cha	100	6.5	98	5.8	101	10.2
Woolb	98	7.5	104	7.8	97	12.0
Tul	103	4.4	102	6.1	95	6.0
Average	101	6.0	101	6.2	98	7.3

(Twelve hours three subjects, nine hours four subjects. m.v. after the group average is, for each table, the average of the m.v.'s, not the m.v. of the averages.)

The sets of eleven were made up to be as equivalent as possible. Any three numbers, as 6, 7, 9, can obviously be combined to form six problems of the type used: 6 times 79, 6 times 97, 7 times 69, 7 times 96, and so on. Five and a half such groups of six would, therefore, make up the materials for a day's test sheet: eleven problems with each of three colors. The problems were distributed among the three colors to give as great variety as possible, and as great uniformity: if 7 was used twice with a color as multiplier those two multiplications did not come together, and there were two sevens as multipliers with each of the other colors. In giving the test, the experimenter simply read each problem to the subject, slowly and distinctly, as "six times eighty-seven." The subject did the work mentally, and then replied with the answer, "five twenty-two." And the experimenter took time from the last syllable he said in giving the problem to the last syllable of the subject's reply.

In combining the results for a half year or more, two important factors had to be taken into account: (1) variations within the hour, and (2) variations from one experimental hour to another through the year. In dealing with practice through the year, some percentage statement of ranking of the colors, within the hour, was naturally suggested. The method finally adopted employed, as norm for the hour, the average of work under all three colors. The time under each color was then rated as a per cent of this.

For instance, suppose on a given day subject A's time for the ten multiplications, under each one of the three colors, ran as follows:

red	green	blue
55.0	57.2	59.2

these three would be averaged, giving 57.1., and per cents taken from this making a score of

red	green	blue
96%	100%	104%

Averages of these per cents through the year gave results free from practice effects from hour to hour. They were free from the many other variations from week to week, due to changes in condition, previous fatigue, and so on. Finally, the results from individual to individual were directly comparable; the average actual time for some individuals was over twice that for others.

The results are summarized in Table II. Mor., who gave results contrary to the tendency of the group in the tapping, does the same here. Otherwise there is a fairly consistent slowing with dim light, and acceleration of work with the bright.

The hue series gave no results so consistent. There is a suggestion of most rapid work with blue and slowest with green. But inspection of the individual averages reveals no consistent tendency such as appears with the brightnesses; minimum rate occurs three times in green, twice in red, once under blue; red and blue are once "tied for first place."

4. *The Effect of Color upon (a) Rate of Free Association, and (b) Immediate Memory for Nonsense Syllables.* The arithmetical work called for associational processes of a rigidly controlled and limited sort. As a contrast to this, it seemed interesting to try what the effect of color might be upon wholly uncontrolled, free association. Some test of memory also seemed necessary for a rounded test programme. Memory for nonsense syllables and rate in free association, under the various hues and brightnesses, were therefore investigated. And, it may be said at once, the results were entirely negative.

The materials and method for the free association test were substantially the same as used by Professor Langfeld in his study of the effects of prolonged fasting.[19] His lists of words, twenty to a list, were used, twenty words with each color. Timing was with a stop-watch. The experimenter simply read each word, slowly and distinctly, to the subject, who was instructed to reply at once with the first word that came into his mind. Time was from the last syllable of the stimulus word to the last syllable of the response. In handling the results, the longest and the shortest times were struck out, as most likely to contain extreme chance variations, the longest as perhaps due to a complex, the shortest as due, perhaps, to a mere perseveration from some previous word or associate. The total for the remaining eighteen words was then found.

The memory test was quite as simple. Immediate memory was clearly the only type which could be controlled adequately for the purposes of the experiment. Various kinds of material were tried; nonsense syllables were found to be the best suited to the test programme. Ten syllables were used with each color; a sample list runs as follows:

zok dib niv bam zek miz niz dop lor tem

In making the lists no long vowels were used (since these were found to "cling" in the memory more), and various other more usual precautions were taken, to make the lists of even and equivalent difficulty.

In giving the syllables they were read at a rate of two per sec., with a grouping in pairs by inflection of the voice. After the first reading the subject was asked to "give back" as many as he could. The list was then read to him again, and he was again asked to repeat all he could remember. This was done twice more, making four readings in all with each list. The syllables were scored as correct or half correct, in the recall; the final record was the sum of the recalls after each of the four readings. For instance, under red, the number of syllables given back after the first reading might be four, after the second five and a half, after the third seven, after the last eight. The record under the red would then be 24.5. The test was a great bore to the subjects, and yielded results of no value.

5. *The Effect of Color upon Rate of Continuous Choice Reaction.* A measurement frequently obtained, in work of this general nature, is speed in choice reaction. The writer felt strongly, however, that the classical reaction-time experi-

ment was neither suited for inclusion in his test programme nor calculated to get at the effects which were being sought.

It was at first planned to use the standard method, two stimuli and two keys, and the time in sigma for each separate reaction. But the technique was considerable; much time would be required to accumulate a small amount of data; and the measurements would be so minute that the appearance in them of so subtle an effect as the colors might be expected to have would hardly be likely. In fact, such investigation, with negative results, had already been reported.[28]

Besides, the writer had a feeling that the effect of a bright light, for instance, might consist not so much in an actual stimulation as in a "sustaining" of any activity which might be going on. Such an influence should show best on some form of rather monotonous work which could go on for an appreciable length of time in routine fashion; something comparable to work in a factory, where there are a few well understood stimuli and half-automatic responses, occurring over and over. Rivers[31] and Hollingworth[17] both used typewriting. Typewriting could obviously not be used in the present study; but something of this general nature, a continuous series of reactions to simple stimuli, was the type of task desired.

It finally occurred to the writer that instead of single reactions, and a time measurement of each, a continuous series, with timing only of the whole series, could be arranged; such a test would give exactly the sustained routine activity desired, and would also accommodate itself much more easily to the total test programme. The test, as finally worked out, may be briefly described. For signals two telegraph sounders were used; one, giving a sharp, loud click, to the right of the subject's table; the other, sounding duller and less loud, to his left. On the table, mounted on a small block of wood about 5 by 9 in. in size, were two keys, close enough together to be operated by the first and second fingers of the subject's right hand. The subject was simply to press the right-hand key when the right-hand signal sounded, and to respond with the other key to the left-hand signal. And the apparatus was so arranged that correct response on the part of the subject to a given signal automatically brought about the presentation of the next signal. But if the subject responded incorrectly, pressed the wrong key, no new signal appeared, and the subject had to rectify his mistake before he could proceed.

The test thus consisted of a continuous series of choice reactions, the subject setting his own rate and the apparatus exactly keeping pace with him, presenting new signals as fast as the old were reacted to. The test was run 4 min. with each color, 2 min. at the beginning of a color period and 2 min. at the end (taking the place of the tapping test, after work with the tapping test was completed, in the routine)

Highest possible speed on the part of the subject was not desired; he was told rather to work along steadily, much as he would in running a typewriter, and to emphasize accuracy rather than speed. Time was marked in 30 sec. intervals; both time and correct reactions were recorded by electric markers on a smoked drum. Only two choices were used, in order that learning might be easy and practice effects not great, and mistakes not over numerous.

The important, and original, feature of the test was, of course, the apparatus by which reactions and stimuli were thus interrelated. This interrelation was brought about by means of a special adjustment between contacts with each signal and with each key, and contacts on a revolving wheel. The wheel consisted of two brass cog wheels soldered together; bearing on the right hand wheel were the contact connecting with the right-hand signal and the contact connecting with the key to be pressed when the right-hand signal sounded, and bearing on the left-hand wheel were the contact to the left-hand signal and the contact to the key to be pressed when the left-hand signal sounded. The key contacts were side by side (that is, bearing on cogs which were side by side on the wheel); and underneath, making on the next cogs below, were the two signal contacts, also side by side, that is, making on cogs which were side by side. Certain cogs on each wheel were filed out, however, and these were so chosen that one, and only

TABLE III

NUMBER OF CORRECT CHOICE REACTIONS IN TWO MINUTES. IN TERMS OF % OF AVERAGE NUMBER, PER TWO MINUTES, FOR THE HOUR

Brightness Series

	I. (bright)				II.				III. (dim)			
Sub.	First 2′		Second 2′		First 2′		Second 2′		First 2′		Second 2′	
	av.	m.v.	av.	m.v.	av.	m.v.	av.	m.v.	av.	m.v.	av.	m.v.
And......	100	2.0	102	6.9	100	8.5	104	8.9	95	7.4	98	7.6
Cur.......	98	5.5	102	5.0	107	5.2	105	2.8	91	8.3	98	7.1
Dav......	103	4.1	99	3.3	100	4.8	99	4.5	101	4.6	96	6.6
Pep.......	101	2.0	101	4.0	101	1.0	100	4.0	99	2.0	98	4.0
Mor......	99	4.2	99	5.9	98	5.2	102	3.9	99	6.2	102	3.9
Woo......	97	3.4	101	2.5	100	5.9	100	4.0	98	2.9	103	4.3
Av.......	99	3.5	101	4.6	101	5.1	102	4.7	97	5.2	99	5.6

(Twelve hours each subject.)

Hue Series

	Red				Green				Blue			
Sub.	First 2′		Second 2′		First 2′		Second 2′		First 2′		Second 2′	
	av.	m.v.	av.	m.v.	av.	m.v.	av.	m.v.	av.	m.v.	av.	m.v.
Allp......	100	2.9	102	1.7	100	4.6	103	2.0	96	4.7	99	2.1
Br........	101	2.5	98	1.6	99	2.5	100	2.1	101	3.1	99	3.1
Bul.......	100	4.5	101	2.5	101	3.1	103	3.3	104	5.3	91	6.1
Chac......	102	3.1	99	2.1	98	3.2	100	3.3	99	2.9	101	1.6
Cul.......	102	3.0	100	3.0	102	2.0	99	3.0	102	2.0	96	3.0
Woo......	95	4.2	103	2.0	95	6.0	106	3.9	100	3.9	101	4.4
Av.......	100	3.4	100	2.1	99	3.6	102	2.9	100	3.6	98	3.4

(Twelve hours each subject.)

The m. v. in the average row is the average m. v., not the m.v. of the averages.

In handling per cents values up to .5% were dropped.

one, cog was presented at any given point. The result was that one, and only one, signal contact, and one and only one key contact, were making with the entire wheel at any given time.

Suppose, then, the subject responds (correctly) to a previous signal by pressing the proper key, thus closing the circuit, and suppose the right-hand signal contact is making with a cog on the wheel. The right-hand signal will be sounded. At the same time the current will go through a magnet, jerking forward a rachet to engage a succeeding cog on the wheel, so that when the subject breaks the circuit, by releasing the key, a spring action against the magnet will jerk the wheel around one cog's distance. The result will be that the cog which was making with the right-hand signal contact will now be making with the right-hand key contact. And only by pressing the right-hand key can the subject again close the circuit, and proceed further with the reaction series.

The essential feature of the device thus consists of this arrangement by which a given contact on the wheel makes successively with a signal and a key contact, so bringing it about that the reaction called for is the only one which can be completed.* The apparatus, which was mounted on a base about 4 by 12 in. (it could have been built half this size) and stood about 3 in. high, was in a sound-proof box under the experimenter's table. The cogs made in irregular order, so that the subject was given signals sometimes alternately right and left, again two or three in succession left or right, and in various combinations. After some five months of practice one subject began to learn the combinations; the connections were then simply reversed, and so all the combinations changed. Aside from this, there were no evidences of marked practice effects.

The results were first handled simply as total number of correct reactions in the first and the last 2 min.; this was taken as a measure of what might be called "reaction efficiency" under the given conditions. However, since individual differences, and variations from day to day, were marked these figures were converted into per cents of the average for the day, as were the times in the multiplication test.

It should be mentioned that 1 min. practice was given before the hour began; there was nevertheless evidence of practice effects through the hour, especially with certain subjects. And since no convenient way was found for eliminating such constant errors from the results

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* The apparatus can be made to handle three, four or more choices, simply by using three, four or more cog wheels with their pairs of contacts. It can be used with any type of stimulus which can be electrically operated, and with any type of reaction. The arrangement would, therefore, seem of some general usefulness. The mechanism used by the writer was experimented with up to speeds about twice those of the average subject, and found perfectly accurate at such rates.

As has been pointed out to the writer by Prof. W. F. Dearborn, the test is somewhat similar to Seashore's "Psychergograph;"[32] the apparatus above described is, however, much more practicable. A mechanism very similar to the writer's has recently been described by McComas.[33]

they may be supposed to make the mean variation somewhat larger than it should be.

In the brightness series the group average shows "reaction efficiency" to be greatest under the medium light, least under the dim. As before, Mor. is an exception, and this time also Wool. Mean variations are also slightly greater under the dim light. Differences between first and last 2 min. periods are hardly consistent or definite enough to allow any significance to be attached to them; the two periods are probably best considered together. In the hue series the group average is least with blue; red and green show no important difference. It can hardly be said that in either series the results are of any significance, except in so far as certain subjects in the brightness series give findings in agreement with their previous records on the tapping and arithmetic tests. But this consistency would seem of some possible importance.

6. *The Effects of Color, as given in the Introspective Reports.* As has already been said, for the major part of the experimentation no introspection was called for. Introspection was obtained, however, for the first and last hours of each term, in order to study (1) group tendencies and the amount of individual variations in report, and (2) the effects of habituation to the colors. The tables are too complicated to present; the findings may, however, be briefly summarized.

In this work introspection in terms of the Wundtian tridimensional theory was called for.

This was done only after considerable hesitation. But more of a description than a mere indication as to pleasure value was wanted. And if each subject was left to form his own categories, tabular statement and cross-comparison would have been difficult. The subjects were therefore asked, for a time, to report on the colors as pleasant or unpleasant, exciting or depressing, tensing or relaxing. Two degrees of each quality were recognized; a color could be pleasant or it could be very pleasant, or it could be indifferent, neither pleasant nor unpleasant. The work was done with the four-color brightness and the five-color hue series, and was continued four weeks with the first and five with the second (making up on each case one "unit" where each color occurs once in each position through the hour).

The group totals show, for the brightness series, the two bright lights as most pleasant, the dim least. The bright lights are most exacting, tensing; the dim is most depressing, relaxing; indifference centers with the medium light. All of this was to be expected, except perhaps the pleasantness of the very bright light, which was not found unpleasant once in spite of its brightness. In the hue series, green was most pleasant, white next, blue most unpleasant. Red is most exciting and tensing, blue and white are most often called

depressing and relaxing. Judgments of indifference are rarest under red, showing that this hue is usually taken positively, one way or the other.

The variability of a given individual, from one hour to another, was considerable. For instance, And. reports the medium-bright light twice as very pleasant, and twice as wholly indifferent; Cha. finds blue three times exciting, once very exciting, once depressing; Bul. finds white tensing once, relaxing twice, indifferent twice; Lang. finds blue pleasant twice, unpleasant once, indifferent twice.

Individual averages also differ markedly. Bul. finds all the hues and all the brightnesses uniformly pleasant, with green and the brightest light each called very pleasant once. The dim light is for Fin. and Dav. uniformly unpleasant. Cha. finds red very pleasant, Ohl. very unpleasant. Red is for Bul. relaxing, for Dav. very tensing. The results of continued experience with the colors were quite what would be expected. There was a thinning of the feeling, a dropping-away of associative enrichment and toning, a matter-of-course attitude which paid very little attention to the colors one way or another.

Thus one individual, in the first weeks of his experience as subject, reported the bright light as "exhilarating, hard on the eyes at first, but less hard than the dim light," and later in the same period, "pleasant, more awake." The dim light was disagreeable, unpleasant, sleepy, tensing, bothering to the eyes. After three terms with the experiment he speaks of the bright light as "just an ordinary light, a bit pleasant when first turned on, doesn't mind it at all." And at the end of the period he says that there "is nothing special to report, slightly pleasant, that's all." Another subject reports in the first weeks that the bright light is "exciting, livening, like sunlight," the dim is "easy on the eyes, relaxing; doesn't feel like working." At the end of the year introspection on the bright light is simply that "the dirt spots on the paper show up more;" the dim light "tends to put to sleep, a little depressing."

Similarly, in the hue series, at the beginning of the year, a subject reports red "very pleasant, because of its richness," and again, "very pleasant, it is so rich." Blue is "pleasant, reminds one of the sky and clouds," again is "ghastly, unnatural, unpleasant, exciting" (this the following week!). At the end of the year blue is "neutral" and again "neutral in all ways." Red is "perhaps slightly pleasant" and again "slightly pleasant: it came as something of a surprise." Another subject who at first "didn't like the red a bit" and again found it "decidedly unpleasant, tensing, exciting," toward the end of the year called it merely "not quite so pleasant as the others," and "on the pleasant side." Blue was at first "very unpleasant, disagreeable, tiresome;" by June it had become "quite pleasant. rather restful" and again quite pleasant, rather restful."

It may be said shortly that there is, if the introspective reports may be trusted, a definite decrease in affective reaction to both hue and brightness, with habituation.

The results obtained in the systematic experimentation may, then, be briefly summarized.

(1) Results with a tapping test, a multiplication test, and a continuous reaction test, suggest a decrease in function under dim, and an increase under bright light. With the other tests no effect of brightness was observable.

(2) The objective measurements showed no effect of hue, independent of brightness, upon the functions tested.

(3) Introspective estimates indicated marked variability, and marked individual differences, in the feeling-attitude toward brightness, and especially toward hue. A marked decrease in affective reaction to both hue and brightness, with habituation, is the only feature common to all the introspective reports.

IV. Summary and Conclusions

So much, then, for the writer's experimentation. It remains to evaluate these findings in the light of previous work, and to summarize what seem to be the most probable conclusions with regard to the problem.

First to be considered (1) are possible differences among the hues, in their effects upon mental and physical activity. As has already been intimated, the writer feels that there is no adequate evidence in previous work to show any such differences. He has found no such differences in the present experimentation. The common notion that certain hues have a marked influence upon emotional tone and mental work is the result, he believes, of such subtle and pervasive influences, arising from figures of speech, custom, artistic and social convention, and everyday association of certain hues to other sensations of a strong affective toning, as have already been mentioned in the historical review. The misconception may also be due in part to a failure to differentiate the influence of brightness from the influence of hue.

If there is an effect of illumination upon mental work, it would appear, then, (2) that brightness must be the important factor. Certain of the experimental results obtained by the writer have suggested a slowing of mental work under dim light and a stimulation under bright light. Such an effect is at least not incompatible with what little previous work has been done in this field; and the hypothesis of a stimulat-

ing or dynamogenic influence of brightness upon mental work and neuro-muscular tone seems by no means unreasonable. It need hardly be added that, if there *is* such an influence, it is a matter of great general importance, from a practical point of view.

It must, however, be emphasized at once that the writer's positive findings are few and far between, and that their reliability is by no means all that could be desired. The writer has argued that in such a study the interrelation of all the results should be taken into consideration, in judging of the significance of the findings. And he has based his conclusions on (1) the consistency of the results obtained from each subject on each single test, (2) the appreciable agreement among different subjects on the same test, and (3) the consistency of the results from test to test with each subject. But even with such a liberal basis of interpretation, it can hardly be said that the results do more than "suggest" the inferences which have been drawn.

It is also obvious that fifteen minutes of experimentation in a laboratory dark-room tell us little about what might be the effect of the same brightness continued throughout the day, in shop or schoolroom. Fundamentally, the problem is not a laboratory problem, and needs study of a larger sort, in schoolroom or shop. The present study can be thought of as contributing little more than a definition of the problem, and a suggestion of methods. But the writer is convinced that there is here a real problem in applied psychology, well worth more extended study.*

* But not, let it be repeated, in the laboratory! The writer finds himself, after three years of intensive research with mental tests, going over this manuscript with oddly mixed feelings. Not so long ago he returned from a four days' visit to a city of about 11,000 population: during those four days, he personally tested every child of the 1,500 children in that school system. And in contrast, three years of weary, discouraged puttering in a dark room, with a total of twenty-six subjects! He would hardly think of basing any very serious conclusion, now, on less than that many hundred. But by all this he does not mean to imply a loss of interest in the older type of experimental problem. Rather he believes that practically all of the laboratory problems can be handled, in the large, by the "test" method, with such massed cases. He is convinced that recent developments in the field of group-tests are much more important for experimental psychology than is usually realized. The group-test is really, he believes, a new method, of splendid promise, applicable to the entire range of psychological investigation. And it is to the "group-test" that he looks for a final conclusive treatment of the majority of the older experimental problems: that is, majority of those problems which may be worth such treatment.

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A PLETHYSMOGRAPHIC STUDY OF SHOCK AND STAMMERING

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The experimental work here reported was begun in May, 1917, and continued until October, 1918. The intention at the start was to test the organic reactions accompanying stammering, with special reference to Dr. C. S. Bluemel's cerebral congestion theory of stammering, and this object has been kept in mind throughout.

As I was unable to secure as subjects any stammerers who had been trephined and as it has been shown that the changes in volume in the hand or finger represented the changes in all parts of the periphery, the only expressive process studied was the change in volume of either the right hand or the second finger of the right hand.

The introspections of all severe stammerers agree that they frequently experience, when called upon to say something of importance, a great fear that they will stammer if they attempt to speak. This fear sometimes is so intense that the stammerer's mind becomes a blank and he stands immovable and speechless; his eyes bulge from their sockets, the perspiration streams down his back, he opens his mouth and may even tremble, thus exhibiting all the symptoms of real fright. It seemed worth while, therefore, to arouse, in both normal speakers and stammerers, an emotion similar to that of fright by producing shock by such means as the unexpected blast of a shrill whistle, and to compare the reactions of normal speakers to shock with

those of stammerers to shock, and the stammerer's reactions to shock with his reactions to stammering.[1]

Before describing my experiments, it will be well to review briefly the results obtained by other investigators.

History of experimental literature. It is needless to review in detail the investigations included by Anderson (1), Angell and Thompson (3), Shepard (29), Stevens (32) or Weber (35) in their historical résumés. I will begin, therefore, with a summary of the results obtained by all the experimenters who have, to my knowledge, worked on this problem, and I will then review the results obtained by two recent writers whose work has a special bearing on my experiment.

Table 1 gives the direction of the changes in arm, brain and intestinal volume during fright, sensory stimulation, pleasant versus unpleasant emotions or sensations, tension versus relaxation, physical work and mental work, as reported by every experimenter who has to my knowledge worked on this problem, together with the dates when the experiments were performed if they are given (otherwise the date of the publication), the number of subjects used by each experimenter (where given), and the number of experiments performed by each experimenter (where given).

All five experimenters who obtained reactions to fright found that fright almost always occasioned a marked decrease in peripheral volume, with or without a temporary rise when stimulus was given; Küppers alone reported an increase in a small percentage of his experiments. Weber found an increase in intestinal volume. The three who experimented on the brain found an increase in brain volume at some point on the record; Berger found an increase followed by a decrease, Mosso an increase only, Shepard a short increase followed by a brief return to normal, then a long characteristic increase.

Fourteen experimenters found that sensory stimuli, in a very large majority of cases, brought about a decrease in arm volume less marked

[1] It is unfortunate that some American writers have employed misleading definitions for "stammer" and "stutter." Some writers use "stammer" to denote a stoppage of speech and "stutter" to denote the repetition of the first letter or syllable of certain words. Shepard and some other writers use "stutter" to denote both repetition and stoppage, and "stammer" to denote mispronunciation, as in baby talk and lisping. As stuttering usually develops into stammering and as many defects of speech consist of both as first defined, I shall use "stammer" throughout this discussion as absolutely synonymous with "stutter" as defined in Webster's *New International Dictionary*—"to make involuntary stops in uttering syllables or words."

than in fright, and preceded less often by a temporary rise; Lehmann alone reported an increase for touch stimuli and a decrease both preceded and followed by a temporary increase for visual and acoustic stimuli. All three experimenters found a rise in brain volume.

Sixteen experimenters compared the vasomotor changes in the periphery accompanying pleasant versus unpleasant emotions and sensations, and all found that the unpleasant states brought about constriction. Eleven found that the vasomotor changes were the same for pleasant as for unpleasant states; five found the opposite to be the case. As but five experimenters, whose interpretations of their results are questionable, found opposite vasomotor changes for pleasant and unpleasant states in but few experiments on but few subjects, and as eleven experimenters, including the most accurate experimenters we have, found no difference in the reactions to these states in about nine thousand experiments on a large number of subjects, the evidence is overwhelming that all emotions and stimuli, whether pleasant or unpleasant, have a very strong tendency to cause vasoconstriction in the peripheral arteries. As the work done by Shepard is absolutely trustworthy, and as he performed one hundred and eighty experiments on two trephined subjects with reliable apparatus, I think his conclusions that all emotions and sensations cause vasodilatation in the brain during the waking state, may be accepted until we have reliable additional evidence to support the opposite view.

Only three experimenters have compared the bodily reactions during a state of tension with those during a state of relaxation. These all found that tension caused vasoconstriction in the peripheral arteries, whereas relaxation caused vasodilatation.

Only two experimenters have studied the vasomotor changes during physical work. Anderson found vasomotor dilatation in the arm in 75 per cent of the reactions from subjects in good physical condition and in but 30 per cent of the reactions from subjects in poor physical condition. Weber found vasodilatation in both the arm and the brain and vasoconstriction in the intestines, when the subject was not tired, and the reverse in the arm and the intestines when the subject was fatigued. There are not sufficient data upon which to base any conclusions as to cerebral and intestinal vasomotor changes during physical work.

The results concerning changes in peripheral volume during mental work are rather contradictory. Most experimenters made no exhaustive study of this problem but requested a few subjects to solve a few

TABLE 1

EXPERIMENTER	DATE	NUMBER OF SUBJECTS	NUMBER OF EXPERIMENTS	FRIGHT	PLEASANT	UNPLEASANT	STIMULI	TENSION	RELAXATION	MENTAL WORK	PHYSICAL WORK
Arm volume											
Anderson............	1915–7	30	7000		No diff	erence				+ (−)+ 75%	+75%
Angell and McLennan.	1896				+ (−)	−90%				+25% −75%	
Angell and Thompson	1899	2	"Complete"	−	−	−	−			+50% −50%	
Berger...............	1904–7							−		−	
Binet and Courtier....	1896			−	−	−	−			±	
Binet and Henri.......	1895				−	−	−			−	
Bonser...............	1903	12			− (+)−	− (+)−				+−	
Citron...............										−	
Drozynski...........	1910				+ (−)	−					
Fere.................	1887				+	−					
Frankfurther and Hir.	1909						−			−	
Gent.................	1903	3	Few		(+) −	−	−	−	+	−	
Gley.................	1903									−	
Hallion and Comte....	1894						−				
Kuppers.............	1913			− (+)	− (0)	−	− (+)			−	
Lehmann............	1892–9	12	100	(+)−	(−)+	−	(+)−(+)	−	+	(+)− −	

Mosso	1879			−	−	−	−			−	
McDougall	1896		1100				−			+−	
Shepard	1905–6	8	289	{ − { (+)−	−	− (+)	−	−	−+	(+)−	
Shields	1895				−	−	−			−	
Sorbonne					−	−					
Stevens	1905		116			{	−80% + 6%			−57% +29%	
Weber	1910		Few	−	+	−		−		−	+
Weld	1912						−90%				
Wundt								−			
Brain volume											
Berger	1904–7	Few	Few	+−	−	±	+	0		+	
Mosso	1879	3		+	+	+	+				
Shepard	1905–6	2	180	(+)(±)+	+	+	+		+−	+	
Weber	1910	Few	Few			−				+	+
Intestinal volume											
Weber	1910	Few	Few	+	−	+				+	−

\+ Increase.
− Decrease.
+− First an increase, then a decrease.
(+)− A temporary increase followed by a decided decrease.
± Now an increase, now a decrease.
{(+) / − In some cases an increase, usually a decrease.
0 No change.

arithmetical problems in connection with other experiments. Fourteen experimenters reported vasoconstriction preceded, in a few cases, by a brief vasodilatation. Five, all careful experimenters, found sometimes vasodilatation, sometimes vasoconstriction. Anderson was the only experimenter who found more vasodilatations than vasoconstrictions. His results seem the most trustworthy, however, because he used periods of work long enough to secure true reactions; he performed the same experiment upon thirty subjects and could thereby eliminate individual differences; he experimented upon more kinds of mental work than all the other experimenters combined and he performed several times as many experiments as all the other experimenters combined. We may conclude, therefore, that mental work sometimes causes vasoconstriction in the peripheral arteries, but more often vasodilatation. Weber found that mental work occasioned vasodilatation in the intestinal arteries, and three experimenters found that it caused vasodilatation in the cerebral vessels, as might be expected, since blood always flows to an organ which is being actively used.

Angell and Thompson (3) found that a steady strain of attention caused little if any vasomotor change, and that every break or shift of attention caused vasoconstriction, the amount of constriction being roughly proportional to the intensity of the emotion or stimulus that broke in on the attention, and the amount of surprise involved. My results confirmed theirs, as do the results I have seen everywhere in the literature.

Summary of results obtained by other experimenters. The results of the many experimenters reported in table 1 show that those sensory stimuli (whether agreeable or disagreeable), especially shock, which cause a break or shift of attention, occasion vasoconstriction in the periphery and vasodilatation in the brain, the vasomotor shift being the most noticeable after intense, unexpected stimuli, and the least marked after weak stimuli.

Mental and physical work, which call forth a steady strain of attention, often cause a temporary vasoconstriction in the peripheral arteries due to the shift of attention to the work; then, if the work continues, it brings about a very gradual vasomotor shift, much less noticeable than that occasioned by sensory stimuli, more often vasodilatation than vasoconstriction in the peripheral arteries, and always vasodilatation in the cerebral vessels.

Summary of results obtained by John F. Shepard. In *Organic Changes and Feeling* (29), Shepard reported the results of his experiments on

the peripheral volume changes of six normal subjects. Three forms of plethysmograph were employed at one time or another, Zimmerman's modification of Lehmann's plethysmograph, the Hallion-Compte air plethysmograph and the finger plethysmograph described by Lombard and Pillsbury were employed whenever it was possible, and a piston recorder was used with each plethysmograph. The thoracic breathing was registered by means of a Sumner pneumograph connected with a Marey tambour. Among the stimuli used were agreeable and disagreeable smells and tastes, colored lights, deep and shrill whistles, chords and discords on tuning forks, music on the violin and zither, noises, attention to counting marks or a minimal sound, to a touch or to a multiplication, recalling of emotional experiences, listening to amusing reading, etc. The subject's arm rested in the swing which held the plethysmograph. A normal record was run for some time before any stimulus was given and time was allowed for recovery after each stimulus. In some of the experiments the subject and experimenter were in different rooms.

Shepard found that the reaction was almost invariably vasoconstriction, preceded in a very few cases by a temporary vasodilatation. Out of 200 reactions, 187 gave a fall, 7 gave a rise which was probably due to changes in breathing and not directly to the stimulus, and 6 gave a rise followed by a fall, in 3 of which the fall was much more marked than the rise. Twenty-two cases of strain gave a fall in volume; relaxation gave a rise. Agreeable and disagreeable stimuli gave the same reactions.

About 150 clear records were obtained simultaneously of the changes in volume of both the brain and the periphery of a laborer of fully average intelligence who had met with an accident which necessitated the removal of a piece of the skull bone, roughly 8 by 6 cm. in area, on the right side of the head near the Rolandic region. A brain plethysmograph was made by attaching a piece of cork, cut to fit the dip over the trephine, to the rubber membrane of the tambour; this was held on the head by means of two bandages. The same stimuli named above were employed and it was found, in general, that all agreeable or disagreeable stimuli, all sensory attention or attention to arithmetical problems, all agreeably exciting light or music, all depression, and the strain of expectation when a neutrally toned stimulus was announced before it was given, gave a fall of volume of the hand with smaller pulse and more rounded dicrotic, and rise of volume of the brain with larger pulse, often with dicrotic raised in position and made sharper.

With relaxation, there was at first quite often a fall of the hand volume and rise of that of the brain, then a gradual increase of the hand and a decrease of the brain to normal. With strong stimuli, the reaction often had a double character; the volume of the hand increased first with smaller pulse and then fell quickly to a much lower level with rounded pulse while that of the brain increased at first with large pulse, then decreased nearly or quite to normal and sometimes showed almost an anacrotic pulse; then finally rose markedly with high pulse and gradually returned to normal. Similar results were obtained in similar experiments both from this subject and another trephined subject, and were reported in *The Circulation and Sleep* (30).

Experimental literature on stammering. I am acquainted with but one plethysmographic study of stammering. That is an article by Fletcher (49). He had as subjects nine stammerers between 14 and 24 years of age, the average age being 17; there were five boys, three young men and one young lady. No attempt was made to make a careful diagnosis of each case, hence the reader does not know whether these cases were primarily mental or physical stammering. The apparatus consisted of a kymograph, a Lehmann arm plethysmograph connected with a tambour recorder, two Sumner pneumographs for the registration of both chest and abdominal breathing, connected to tambour recorders, a Jacquet chronometer, a Rousselot microphone, a Deprey d'Arsonval galvanometer, an adjustable reading rack and cards on which were printed various selections of prose and poetry. No attempt to minimize arm movements by a swing or otherwise was mentioned, yet stammerers constantly move their arms while stuttering.

The breathing results are trustworthy and were as follows: The characteristic normal rest-breathing curve showed inspirations and expirations of approximately the same length, and the thoracic and the abdominal curves were approximately synchronous in phase. In general, the stutterers presented no permanent peculiarities of breathing unrelated to the function of speech. There were apparently as many varieties of breathing peculiarities among stutterers as there were varieties of stuttering. Breathing abnormalities tended to become stereotyped in certain forms for certain individual stutterers; they appeared often as temporarily adopted expedients to help out in the beginning of speech. The thoracic curve seemed to be more sensitive to mental disturbances than the abdominal curve. In many cases the thoracic and abdominal curves tended to approach each other, often to the point of touching; this sometimes continued through-

out the speaking interval. In many cases breathing disturbances appeared before the speaking interval and continued for as long as eight seconds after it. The ratio of the time of inspiration to that of expiration was found to be 0.217 second during normal speech and 0.535 second during stuttering. The expiration interval averaged 24.6 seconds during stuttering, of which the vocalization interval occupied but 9 seconds; the record of normal speech showed that of an average expiration interval of 26.8 seconds, 25.6 seconds were utilized in vocalization. There appeared to be an efficiency of 90 per cent with normal speech as compared with an efficiency of 36.5 per cent with stuttering.

Shepard's plethysmographic results were as follows: There was a marked attention drop (constriction) in 73 per cent of the cases where the stutterer was told to speak or read, this drop being greater in the latter case. Immediately after the attention drop there began, in 90 per cent of the records, a rise (vasodilatation) which usually lasted until the subject ceased stuttering; this amounted to a rise of 5 cm. in 31 seconds in one extreme case. In 62 per cent of the records, this rise was interrupted by irregularities in addition to those due to breathing and movement. When subjects were asked to imagine themselves in situations in which they would be likely to stutter, the curve showed slight vasomotor constrictions in certain cases, never vasodilatation. Both the amount of the general rise and the distortions of the plethysmograph are correlated with the degree of severity of the stuttering.

The speech of the stutterers was attended by an abnormal acceleration of the pulse rate. At a point in the period just prior to the speaking interval, the pulse rate averaged 90.2, ranging from 72 to 120; at a point at the beginning of the speaking interval, the pulse rate averaged 99.8, ranging from 78 to 126; and at a point at the close of the speaking interval, the pulse rate averaged 98.6, ranging from 72 to 129. It will be remembered that the normal pulse rate in adults ranges from 70 to 75.

Apparatus. The apparatus used in these experiments was practically the same as that used by Dr. John E. Anderson, and consisted of a Zimmerman kymograph, a Verdin pneumograph, a Sumner pneumograph, a Marey tambour, a Lehmann plethysmograph, a finger plethysmograph, a piston recorder and two small electromagnets.

The kymograph was the regular Zimmerman model with an extension which permitted the use of smoked paper 2.5 m. long by 16 cm. wide. This kymograph was practically noiseless at the low speed at which it was run. As few of my records required smoked paper over

8 cm. wide, I usually went twice around each record; this enabled me to perform four consecutive experiments of approximately an hour each without changing records.

Zimmerman's modification of Lehmann's arm plethysmograph was used in the earlier experiments. A long-sleeved rubber glove was substituted for the blind sleeve, and a movable but tightly fitting round block of wood was placed vertically within the plethysmograph near the end furthest away from the part where the rubber glove was attached. The plethysmograph was attached firmly to a swing suspended from the ceiling. An elbow-rest, which could be moved along a groove in the swing and clamped at any point, kept the subject from withdrawing his arm; a leather strap, passed over the subject's forearm near the elbow and beneath the swing, kept the subject from raising his arm out of the elbow rest and thus withdrawing it slightly from the plethysmograph; and the vertical stick within the plethysmograph, when lightly clasped, kept the subject from thrusting his arm further into the plethysmograph. Thus the only motion which could seriously affect the record was a motion of the arm within the plethysmograph, a movement which could usually be detected on the record because of its characteristic abrupt rise or fall. The curve often returned quickly to normal after such movements and was then little affected; if it did not return to the normal level, that part of the record was always discarded. The subject was instructed always to clasp the stick lightly, because it was found that there was a tendency to relax the grip now and then when the stick was clasped tightly, in which case the recorder seldom returned to the normal level. He was told, also, to relax his muscles and to keep his hand in as comfortable a position as possible.

After the subject put on the rubber glove of the plethysmograph but before he thrust his entire forearm into the plethysmograph, the operator filled the plethysmograph with water heated a few degrees above bodily temperature and screwed on the glass tube to the top of the plethysmograph. This glass tube was 24 cm. long and, when nearly filled with water, served to press the water tightly against the subject's hand at every point. The subject then thrust his arm into the chamber until he clasped the stick lightly, and the operator adjusted the elbow-rest and strap and added or took water from the glass tube until it was filled to within 2 cm. of the top. The top of this glass tube was connected by thick-walled rubber tubing, through a syringe and outlet to the air, with the piston recorder, as described on page 296 in connection

with the finger plethysmograph. The piston of the recorder used with the arm plethysmograph was cut with shears from a thin sheet of brass and was therefore not perfectly round. A strip of bond paper with a fine wire inserted at the outer end was used as a recorder. The lever of the recorder was the same as that to be described later, but it worked up and down between the two springs made of 36 gauge wire (0.019 cm. in diameter) used by Doctor Anderson, touching the springs about 2 cm. from their axes. The lever worked at every point against the delicate springs so that when the pressure against the piston was relaxed, the piston automatically returned to its normal level, thus counteracting the effects of inertia and momentum. By turning the strip of brass to which the springs were attached about the axis, the springs could be set at any distance from the axis of the recording lever; hence the size of the recorded pulse could be regulated for differences in each subject. As this arm plethysmograph could not be used with stammerers, since they moved their arms continuously while stammering, the finger plethysmograph was substituted in my later experiments, in order that I might compare more readily the vasomotor changes in stammerers with those in normal speakers.

The finger air plethysmograph was made in the shop at the laboratory and was a modification of the one used by Lombard and Pillsbury and described in volume III, pages 191–193, of this Journal. The air chamber consisted of a brass pipe 10 cm. long and 2.5 cm. in diameter with a metal disk, pierced by a pipe with a bore 4 mm. in diameter soldered to one end, and a rubber diaphragm 0.026 inch thick, with a small hole in the center, fastened to the other end. This air chamber was placed inside of, and projected 1 cm. in front of, a second watertight pipe, 12.5 cm. long and 4.5 cm. in outside diameter, with two outlets through which water of constant temperature might pass and keep the air in the air chamber at constant temperature. As there was no running water in the room, the water chamber was not used. This plethysmograph was firmly attached to a swing suspended from a rod 43 cm. above it. When the subject thrust the middle finger of his right hand through the hole in the rubber diaphragm until it could go no farther, the operator adjusted the same elbow rest and strap used with the arm plethysmograph. Then no ordinary movements could affect the record other than movements of the finger within the plethysmograph; these could usually be readily detected because of their characteristic abruptness. As the recorder usually returned to its normal level after such movements, they seldom affected the record;

the few pairs of records in which the lever did not return to its normal level were discarded. A thick-walled rubber tube connected the finger plethysmograph with a metal T-tube. One end of the T-tube led to a glass syringe 10 cm. long with a piston 13 mm. in diameter. The other end of the T-tube led through a thick-walled rubber tube of the same diameter to a second similar T-tube. One end of this second T-tube led to the open air and was always open when no experiment was in progress, but always closed by a metal clamp when the experiment was in progress, and the other end led to the piston recorder. As the syringe was airtight it was possible by moving the plunger to alter the [illegible] in the system of tubes connecting the finger chamber with the recorder and thus place the lever in a horizontal position when it was working too high or too low because of changes in temperature [illegible].

[illegible] apparatus which have been devised for recording volume [illegible] changes a piston recorder properly made is the most delicate. As the piston recorder which I used with the [illegible] [illegible] made to record when attached to the finger plethysmograph it was necessary to modify this recorder.

[illegible] a glass T-tube having a bore 5.5 mm. in diameter which was almost round. One of the opposite ends was sealed, the other was ground [illegible] so as to be perfectly round its full length. [illegible] the piston [illegible] a perfectly round piston, which [illegible] [illegible] and worked up and down and turned [illegible] in the tube without binding, was cut from a strip of celluloid [illegible] inch thick. The celluloid piston was practically as stiff as one made of sheet brass, was soft enough to be cut by a hollow round steel punch. This was a decided advantage because brass has to be cut with shears and will necessarily have little corners which will permit [illegible] the record. The point of a stiff steel [illegible] inch in diameter was passed through the center of this piston [illegible] slightly [illegible] so that it could not pull off; the head was cut off [illegible] was made of this end. When this hook was passed through a hole which was drilled in the brass lever and which just fitted [illegible] [illegible] very satisfactory piston was [illegible] cm. in length.

The lever of the piston recorder was the one used by Dodge and [illegible] [illegible] the writing lever made of bond paper tipped with [illegible] was replaced by a very thin strip of celluloid 0.5 mm. [illegible] thickness which was shaped at one end to form a sharp writing [illegible] [illegible] to the short brass part of the lever. [illegible]

celluloid lever was almost as flexible horizontally as the bond paper but it had greater vertical rigidity, could not be bent by accident as could the paper and was not affected by air currents as was the paper. All of the lever but the celluloid tip, which was 54 mm. long and 4 mm. wide, was cut from a piece of sheet brass 0.018 inch thick. It was made from a strip 13 cm. long and 4 mm. wide, and one end was turned twice at right angles and fastened to its axis by the pin *MN* as shown in figure 1. A small piece of lead, *L*, was placed on the arm of the lever on the opposite side of the axis from the writing point; thin shavings were cut from this lead until the piston would stay at any point of the barrel where it was placed, before oil was dropped into the barrel or the writing tip touched the drum of the kymograph. Thus the pressure on the lower part of the piston had to work only against the weight of the drop of heavy machine oil on the top of the piston and the friction of the joints and of the writing tip of the lever on the smoked paper; this proved just sufficient in an upward thrust to diminish the extra upward movement due to momentum from the strong upward thrust at the beginning of each pulse beat. As the weight of the drop of oil and the friction on the smoked paper act in opposite directions when the piston is falling, the lever quickly returned to normal after each pulse beat. I watched the piston closely during all experiments and seldom saw a bubble of air escape unless a little oil hardened on the sides of the piston-barrel and did not permit the piston to move freely. I avoided this by frequently removing with alcohol the oil which is bound to adhere to the sides of the barrel as the piston works up and down. What little oil ran down the sides of the tube collected at the sealed end directly below the piston-barrel and did not interfere with the pulse waves which came in through the third horizontal arm of the T-tube from which the recorder was made. The piston could move an inch above or below the position it assumed when the lever was horizontal, without catching or binding.

The springs used with the arm plethysmograph were discarded as the changes in pressure due to the minute vasomotor changes in a single finger were not sufficient to move them. The writing point was 12.5 cm. from the axis of the lever and the piston rod was attached to the sheet brass part of the lever 3 cm. from the axis; hence the motion of the piston was magnified a little more than four times on the record. When the piston rod was attached nearer to the axis of the lever, the piston was apt to bind in its barrel, and the piston rod often touched the sides of the barrel and bound when the lever moved much above or

below the horizontal position. If the lever were much longer, the friction between the writing tip and the drum would increase and interfere with the free working of the piston within its barrel.

The piston recorder was fastened securely to the end of the round horizontal rod *AB* by the clamp *A*. This horizontal rod was fastened by a clamp (not shown in the figure) to a vertical rod which was firmly attached to the horizontal revolving arm of a stand which could be raised or lowered by turning a screw, and placed nearer or farther away from the drum of the kymograph by moving it gently with the hand. At a convenient distance from the end of the horizontal rod *AB*, a vertical hole, *B*, was drilled to fit the smaller round rod *BQC*

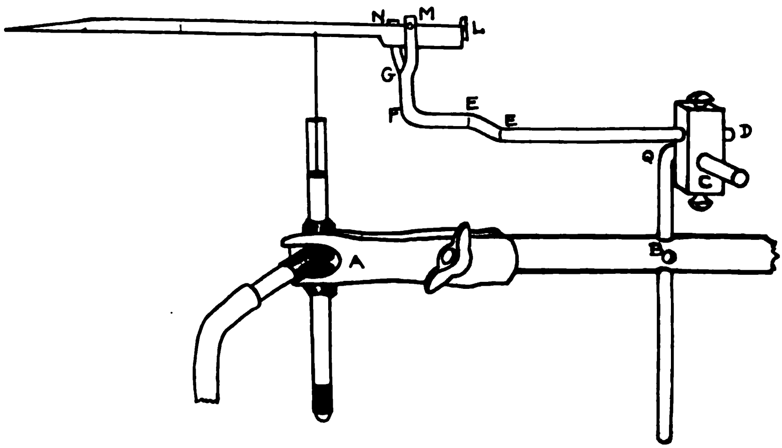

Fig. 1

which was turned at right angles at *Q*. Two horizontal holes, *C* and *D*, were drilled at right angles to each other in the short square rod *CD* to fit the round rods *BQC* and *DEFG*. Rod *DEFG* was turned horizontally at right angles twice at *E* and vertically at right angles at *F* and was split down the middle and bent at *G* to hold the axis pin *MN*. Screws clamped the rods firmly together at *B*, *C* and *D*. The piston could be raised or lowered to any point of its barrel by loosening the screw *B* and raising or lowering the vertical rod *BC*. The piston rod could be brought over the center of the piston barrel and made parallel to this by loosening screws *C* and *D*, moving the rod *DEFG* and revolving it about the axes *QC* and *DE*. When the lever is horizontal, the apparatus should be so adjusted that the piston-rod will lean

slightly toward the writing point because it swings away from the writing point with either an upward or downward movement, and such an inclined position keeps the average position of the piston-rod more nearly vertical and prevents binding.

This piston recorder satisfied the requirements outlined by Lombard and Pillsbury on page 187 of volume III of this Journal. The piston was thin so as to minimize the friction in the piston barrel. It was of small diameter and so connected to the piston rod and the lever that it could not bind and could tilt but little. The movable parts were as light as possible in order to avoid the effects of inertia and momentum. The piston moved freely in the tube and yet followed readily the movement of the lever, which not only described an arc as it moved up and down, but moved a little to one side or the other when the writing point passed over an uneven place on the smoked paper, so as not to keep exactly over the center of the piston. As the compressibility of the air below the piston requires that the lever should write on the smoked surface of the kymograph with the least possible friction, the lever was made flexible so as to yield to the inequalities in the recording surface and at the same time be sufficiently springy to maintain a continuous, gentle pressure on the drum of the kymograph.

The lever of the piston recorder worked up and down in a vertical plane parallel to the drum of the kymograph. The writing point should be tangent to the drum when the piston is about 15 mm. above or below the position it assumes when the lever is horizontal. If the writing point is tangent when the lever is horizontal, it will not, when it rises or falls, touch the drum more than a few millimeters, unless, when horizontal, it presses too tightly against the drum to make an accurate record.

To test the effect of this change in pressure on the drum as the writing lever approaches and recedes from the drum, and to see what change in volume of the finger corresponds with the rise or fall of 1 mm. on the record, I constructed the following piece of apparatus. With thick-walled rubber tubing I connected one end of a glass capillary tube, 10 cm. long, with a bore 2.25 sq. mm. in cross section, with the part of the T-tube which led to the air in my former experiments. I attached about 30 cm. of the same rubber tubing to the other end of the capillary tube and passed it between a board and the table. I clamped the capillary tube in a vertical position close to a scale and filled it with mercury. By tightening or loosening the clamp which pressed the board against the table, I could raise or lower the height of

the mercury in the capillary tube. As I made the volume of air from the mercury to the T-tube the same as that in the tube which formerly led to the air, and placed in the plethysmograph a wooden finger of the same volume as my own finger, the volume of air in the plethysmograph and connecting tubes was the same as it was in my former experiments; the wooden finger, of course, could not move. When this apparatus was connected, I lowered the mercury by eight equal amounts and then raised it by the same number of equal amounts, running the kymograph for an instant between each rise or fall. I did this several times. Then I measured the height of each little rise or fall on the smoked paper and found that these all averaged the same length for a given position of the lever, and that the position of the writing lever therefore made no difference in accurately recording the vasomotor waves when it just touched the drum when the piston was raised just a little more than 15 mm. above the horizontal position of the lever. When the writing point just touched the drum when the piston was raised considerably more than 15 mm. above the horizontal position of the lever, the pressure of the writing point on the drum became so great, when the lever was horizontal, that the mark then traced was much shorter than that traced when the writing point just touched, although both represented the same change in volume. It is necessary, therefore, to move the writing point nearer the drum if the writing point rises or falls much more than 15 mm. from the horizontal position, and to move it away the same amount when it returns.

I repeated this procedure when the plunger of the syringe was at one end of the syringe and again when it was at the other end, and found that no difference in amplitude could be detected in the record for like changes in the column of mercury at these two positions; hence the differences in the position of the plunger of the syringe in different experiments, and differences in the sizes of the fingers of different subjects which were in all cases less than the volume of the syringe, do not affect a trustworthy record of the vasomotor changes.

I found that a rise of 1 mm. on the records always denoted an increase in volume of the finger of 3 cu. mm., and that a fall of 1 mm. on the record always registered a decrease in finger volume of 3 cu. mm., for changes not exceeding 15 mm. above or below the horizontal position of the lever, and for all changes if the lever is moved a little toward the drum when the rise or fall exceeds 15 mm. and is withdrawn the same amount when it returns to that point.

As my piston recorder appeared to move instantly when I moved the plunger of the syringe, and as the latent period of a similar piston recorder tested by Lombard and Pillsbury was found to be but one one-hundredth of a second, I did not repeat their test. A latent period of less than a second would not affect my results.

As Lombard and Pillsbury found by careful experiment that the curve traced on the record by a piston recorder similar to mine was a true record of the normal pulse curves from the finger, and does not originate in the recording mechanism, I shall assume the same to hold true with my recorder without repeating their elaborate test experiment.

The thoracic breathing was obtained either by a Sumner pneumograph or by a Verdin pneumograph. The connecting chain was passed just below the subject's armpits. As the Verdin pneumograph frequently turned over during the stammerers' contortions and gave a less magnified record than the Sumner pneumograph, the Sumner pneumograph was used in practically all of the records so that the records of normal speakers and stammerers might be more easily compared.

The breathing record was obtained by a writing lever attached to a Marey tambour 2.5 cm. in diameter. The lever was well balanced and consisted of a bond paper writing arm with a very fine wire at its outer end. As the tip of the writing wire was 14.3 cm. from the axis of the lever, and the lever was attached to the rubber diaphragm of the tambour 1.2 cm. from the axis of the lever, the movements of the tambour were magnified twelve times. As the rubber diaphragm of the tambour was uppermost in my earlier experiments, the top of the curve in these records represents empty lungs. As the rubber diaphragm was inverted in my later experiments, including most of the work with the finger plethysmograph, the top of the curve represents full lungs in these records; this seemed more logical and made the records of the stammerers easier to read. Unless specifically mentioned to the contrary, the top of the records here reproduced will represent full lungs and the bottom of the records empty lungs.

The breathing record is a valuable check on the plethysmographic curve as it shows what sudden changes in the plethysmographic record are caused by deep breathing, yawning, laughter or sneezing. The breathing record was especially useful in comparing the breathing of stammerers and normal speakers while reading the same passage. I used the pneumograph only for this purpose.

A thin celluloid writing lever, pointed at the outer end, was attached to each of the electromagnets. The upper magnet was connected with a two-way switch. One side of this switch was connected with a time key which could be pressed by the operator and with another key which could be pressed by the subject. The other side of the switch was connected with one side only of a large electric pendulum making one stroke a second; this caused the magnet to click and mark the record every two seconds. When the clock happened to be out of order, I used a stop watch and made marks on the record at known intervals by pressing the operator's time key; as the kymograph rotated at practically a uniform rate, the time could be interpolated pretty accurately between any two points on the record. The lower electromagnet was connected *a*, with the operator's signal key; *b*, to a key which could be pressed by the subject; *c*, in series with a signal light in front of the subject through a switch near the operator and *d*, in parallel with a faint signal buzzer near the subject through another switch near the operator; thus signals were recorded on the record the instant they were given, unless the operator had to use one hand to make the stimulus, as in blowing a whistle, and press his signal key with the other hand, when there was apt to be an error of a fraction of a second. Such an error, however, was too small to influence the results, as I did not try to make any measurements in fractions of a second, whole seconds being accurate enough for the purposes of these experiments.

Procedure. The arrangement of the apparatus used is shown in the accompanying photograph. The writing points of the four recorders were adjusted to write in the same vertical line when the recording levers were horizontal. The time recorder was at the top, the operator's signal recorder next below, the tambour recorder which traced the breathing curve below that and the piston recorder, which traced the vasomotor changes, was at the bottom. I adjusted the swing at a comfortable height for the subject's arm and then adjusted the elbow-rest. Next, when the subject's lungs were half full of air, I put on the pneumograph so that its writing lever would be above the horizontal position as much of the time as it was below it, and thus work more nearly straight up and down. After requesting the subject to remain quiet until the experiment was over, I then closed the outlet from the tube which connected the finger plethysmograph with the air. When the writing point of the piston recorder remained still for a few seconds, showing that the temperature within the tubing had

ceased to rise noticeably, I started the kymograph. As I was interested in the volume changes rather than in the shape of the pulse, I ran the kymograph at low speed; hence little, if any, pulse waves are noticed in these records. Very distinct pulse waves appeared in some subjects, however, whenever the kymograph revolved at high speed. I ran the kymograph for about a minute before any stimulus was given, in order to obtain a normal record from which to measure the reactions to the stimuli that followed; during this normal period I asked the subject to remain as calm and as quiet as possible so that no emo-

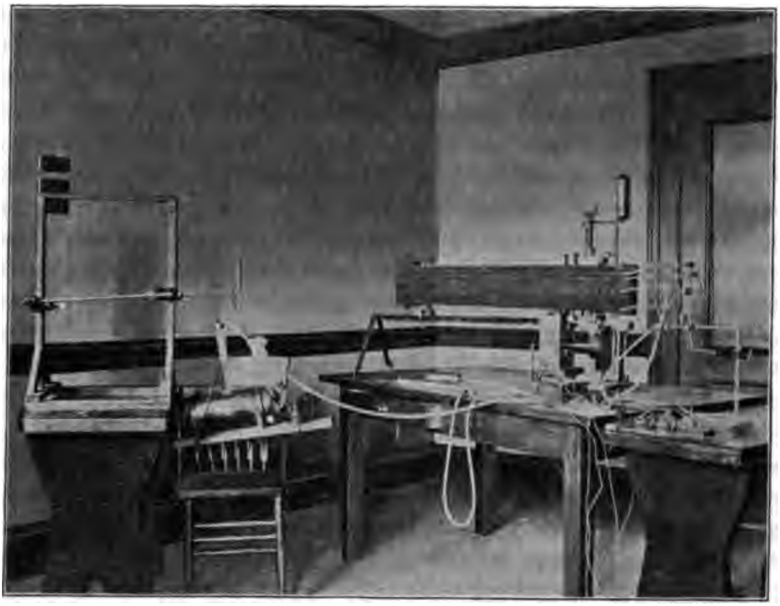

Fig. 2

tions or movements would affect this normal reference line. After the stimulus was given, at least a minute was allowed for the piston recorder to return to its normal level, unless it returned much sooner. Sometimes several stimuli were given in quick succession; sometimes stimuli were given at intervals of a few minutes; sometimes but one or two stimuli were given in an entire period of an hour. The kymograph was stopped at intervals to secure reference lines from the four recorders, to make sure they were keeping in a vertical line. The subjects were allowed to rest and to move whenever they became tired

or uncomfortable or whenever their records showed frequent arm movements; but first they always asked the experimenter's permission to move. A screen was always placed between the experimenter and the subject so that the subject could not see what stimulus was next to be given and the operator was careful not to make a noise when handling stimuli.

I had, as subjects, thirteen graduate students or college seniors and ten stammerers. The normal speakers were experimented upon once each week at the same hour, between 9 a.m. and 1 p.m.; three came for two months, six came for four months and four came for eight months. Nine were men, four were women. The stammerers, all young men with the exception of one boy, came at irregular intervals, frequently in the evening after a hard day's work; some came but once or twice. I selected from about one hundred students at the Boston Stammerers' Institute those who could be depended upon to stammer whenever they were called upon to read and those who did not move their arms violently while stammering; a large majority of the pupils could not be experimented upon. As most of the pupils experimented upon learned to read without hesitancy within two weeks from the time they entered the Institute, they could not long be experimented upon as stammerers. Some came to the laboratory at various intervals during their cure, so that I could study the vasomotor changes at different parts of their cure and compare them.

I gave, in all, about 1000 stimuli. A large majority of the records had to be discarded because they were obscured by arm movements. Less than 1 per cent of the reactions to stimuli with the Lehmann plethysmograph were comparatively free from arm movements. About 150 reactions to stimuli were retained and measured.

I employed the following method to obtain the fall or rise in millimeters of the recording needle during an emotion, the time in seconds from the stimulus to the lowest or highest point of the curve traced immediately after the stimulus was given and the time in seconds from this lowest point to the return of the needle to normal. I placed a transparent celluloid triangle over the record and moved it around until I got one edge over what seemed to be the route the needle would have taken if the stimulus had not been given, allowing for all arm movements as far as possible. If I did not wish this part of the record for reproduction, I then drew a line with a fine needle along this path which the recording needle would have traced if no stimulus had been presented; if I wished to preserve the record for publication, I left the

triangle lying there. The line traced by the needle was never parallel to the time line, for the tube was usually either warming up a little or cooling down a little even when I tried to keep the room temperature constant. The lowest or highest point of the curve was obtained by placing a second triangle touching the first before it was moved from this position, and sliding the first triangle along the second until it became tangent to the lowest or highest point of the curve. This position of tangency was marked on the record with a sharp needle. I next placed a triangle along reference lines taken at intervals on records to ascertain whether the ends of the piston recorder, tambour recorder and two electromagnet recorders were keeping in a straight vertical line, so as to be tangent to the reference marks traced by the operator's key and the piston recorder. I placed a second triangle touching the first and slid the first along the second, marking on the time line the point corresponding with the instant the stimulus was given, the point corresponding to the moment when the fall of the needle was at its maximum and the point corresponding with the instant when the needle returned to normal. By counting the time notches between these marks, I obtained the two times referred to above. By placing one leg of a right triangle on the time line with the other leg passing through the lowest point of the plethysmogram, I was able to measure along the latter leg, with a millimeter scale, the drop to the lowest point of the curve from the standard line the needle would have traced if no stimulus had been given. If an arm movement was noticed and the needle did not return to this standard line within a minute from the time the last stimulus was given, that part of the record was discarded. Arm movements could usually be readily distinguished because of their abruptness. If a record contained arm movements at intervals of an inch or two, I discarded that part of the record, because these movements showed that the subject was then too nervous to keep still, so that it was impossible to get an accurate reference line.

Among the stimuli employed at one time or another to arouse emotions in both normal speakers and stammerers were the following:

1. A shrill whistle.
2. A loud crash, sometimes in front of the subject, sometimes behind him.
3. A sudden yell from the operator.
4. The report from a cap, sometimes placed in the book the subject was asked to read so that it would go off when he opened the book.

5. The sudden entrance of a third person into the room.
6. A loud sneeze by the operator.
7. The subject read silently an exciting story or gruesome description and pressed a key whenever he experienced an emotion.
8. The operator let down an artificial spider on a string close to the subject's head.
9. The subject was directed to take an unpleasant unknown object, such as a rubber snake, out of a cloth bag.
10. The operator threw a light, artificial rubber snake at the subject.
11. The operator aimed at the subject a noisy, self-propelled red balloon.
12. The operator placed close to the subject material for a bonfire, oiled it in his presence and told him to open the match box given him at a signal and light the fire. (When the subject pressed the catch to open the match box, a spring made it fall apart.)
13. A large mechanical bug crawled toward the subject.
14. The operator set near the subject a mechanical frog adjusted to jump six feet into the air about half a minute after being placed.

RESULTS

As arm movements affected most of the curves traced by the piston recorder when it was attached to the arm plethysmograph, no records made with the arm plethysmograph are included in the following tables. Although these curves could not be accurately measured and compared, they confirmed the results tabulated below.

All of the curves showed vasoconstriction in the finger both during stammering and during the emotional disturbance induced by a loud, sudden stimulus such as a shrill whistle, which will hereafter be called *shock*. A small proportion of the curves showed a slight rise for three or four seconds, followed by a decided fall of considerable duration.

In all of the tables, "fall in millimeters" was measured from the path the writing needle of the piston recorder would have taken had no stimulus been given, to the lowest point of the curve immediately following the stimulus. If, during a long period of stammering, there was a temporary rise followed by a greater fall than the first one, the greater drop was the one measured. Figures 4 and 6 show the method of measuring these falls. "t" was measured in seconds from the instant the stimulus was given to the time when the recording needle

first attained the maximum drop. "T" was measured in seconds from the end of "t" to the time when the recording needle first returned to the path it would have taken had no stimulus been given, unless the time of maximum drop occurred during a long period of stammering, when "T" was measured from the end of the stammering period instead of from the end of "t." Under each of the headings "fall," "t" and "T," the maximum, the average, and the minimum are given, so that individual differences may be studied.

Table 2 shows the vasomotor changes in seven normal speakers during shock induced by the six kinds of stimuli named in table 3. These subjects are listed in the order of decreasing magnitude of fall. This

TABLE 2

Vasomotor changes in individual normal speakers accompanying shock

SUBJECT	NUMBER OF STIMULI	FALL IN MILLIMETERS			t IN SECONDS			T IN SECONDS		
		Maximum	Average	Minimum	Maximum	Average	Minimum	Maximum	Average	Minimum
E	5	26	21	15	16	10	5	100	56	33
G	15	20	13	5	22	11	2	95	35	7
H	12	19	10	5	43	14	3	98	36	13
A	9	13	8	5	44	23	6	75	47	16
J	13	15	8	3	28	14	8	98	39	10
F	9	20	6	2	70	31	7	64	31	11
D	4	9	4	1	51	32	23	64	37	12
Total	67									
Average		17	10	5	39	18	8	85	39	15

table shows that these subjects are also arranged approximately in the order of increasing length of "t," but in the order of decreasing length of "T" the correlation between the average "Falls" and "t's" being −0.775, and the correlation between the average "Falls" and "T's" being +0.908. The order is the same, in practically every case, both for the maximum and for the average. This table shows, therefore, that for the subject who has a relatively large fall, the time of fall is relatively short; for example, subject E's average fall of 21 mm. occurred in an average of 10 seconds, whereas subject D's average fall of 4 mm. occurred in an average of 32 seconds. This table shows, furthermore, that those subjects who experience the largest falls also have the slowest recovery and the individual cases show that the greater the fall,

the slower is the recovery. A study of the individual cases shows that subjects do not vary any more from day to day than they vary during a single hour. As table 3 indicates, these differences for the same

TABLE 3

Vasomotor changes in normal speakers accompanying shock brought on by stimuli of different intensity

STIMULUS	NUMBER OF STIMULI	NUMBER OF SUBJECTS	FALL IN MILLIMETERS			t IN SECONDS			T IN SECONDS		
			Maximum	Average	Minimum	Maximum	Average	Minimum	Maximum	Average	Minimum
Gun	5	4	26	18	7	34	14	2	100	63	10
Whistle	10	5	18	9	5	24	12	6	80	30	15
Threw snake	5	3	15	10	6	38	14	6	60	40	13
Crash	19	7	23	9	1	51	19	2	98	34	11
Yell	3	3	7	6	5	70	31	10	40	29	23
Spider	7	5	11	7	2	23	15	6	72	31	12
Total	49	7									
Average			17	10	4	40	17	5	75	36	14

TABLE 4

Vasomotor changes in stammerers accompanying shock brought on by stimuli of different intensity

STIMULUS	NUMBER OF STIMULI	NUMBER OF SUBJECTS	FALL IN MILLIMETERS			t IN SECONDS			T IN SECONDS		
			Maximum	Average	Minimum	Maximum	Average	Minimum	Maximum	Average	Minimum
Gun	7	5	30	25	9	16	10	5	110	80	38
Whistle	11	5	30	19	8	20	12	7	88	44	19
Threw snake	5	5	30	15	0	19	13	9	65	48	37
Crash	6	5	23	14	9	65	21	6	93	50	25
Yell	6	4	25	16	10	19	12	8	58	43	30
Spider	4	4	13	8	3	11	7	4	34	28	21
Total	39	10									
Average			28	17	6	25	13	6	74	50	28

subject are due to the intensity of the stimuli and to the degree of surprise involved. All these tables show decided individual differences.

Table 3 shows the reactions of the seven normal speakers listed in table 2 to six stimuli of different intensity. Table 4 shows the reac-

tions of ten stammerers to the same stimuli. These tables show that the more intense the stimulus is, the greater is the vasoconstriction and the slower is the recovery. A close examination of the individual curves showed that the more unexpected the stimulus is, the greater is the vasoconstriction and the slower is the recovery; this is seen when the reaction to the first gun fired is compared with that to later reports of the same gun, or when the whistle is blown several times on the same morning and the successive reactions are compared, or when the introspections of the subjects tell which records to compare. These tables also show that stammerers as a class experience greater and more rapid vasoconstriction and slower recovery in shock than do normal speakers.

TABLE 5

Vasomotor changes in individual stammerers accompanying shock

SUBJECT	NUMBER OF STIMULI	FALL IN MILLIMETERS			t IN SECONDS			T IN SECONDS		
		Maximum	Average	Minimum	Maximum	Average	Minimum	Maximum	Average	Minimum
Web	7	50	26	8	11	9	5	100	59	29
Wi	7	30	23	9	12	10	7	110	66	35
Ad	4	23	20	14	16	15	11	93	65	30
De	3	23	14	8	20	19	19	58	41	19
Li	5	18	12	10	9	8	6	65	42	30
An	4	15	12	10	14	10	7	75	38	22
Fa	1	12	12	12	65	65	65	25	25	25
Bo	2	8	6	3	13	12	11	42	34	27
Total	33									
Average		22	18	9	20	13	16	71	52	27

Table 5 shows the reactions of eight stammerers to shock produced by the six stimuli listed in table 4. This table, like table 2, shows that those stammerers who experience a relatively great vasoconstriction also require a relatively long time for recovery, the correlation between the average "Falls" and "T's" being +0.698. It shows, also, that those subjects who experienced relatively great vasoconstriction tend, like normal speakers, to have relatively rapid vasoconstriction, although the correlation between the average "Falls" and "t's" is but −0.039. This is not so striking as for the normal speakers, because fewer stimuli were given and the stammerers had not been trained to keep their arms still, as had the normal speakers; nevertheless this relationship is noticeable in the first four subjects listed. The stam-

merers experienced 80 per cent greater vasoconstriction than did the normal speakers, and this attained its maximum in two-thirds the time it did in normal speakers. The time of recovery was 23 per cent longer for the stammerers than for the normal speakers. The maximum fall for stammerers was almost double the maximum fall for normal speakers, and the minimum time of recovery was nearly three times as long for the stammerers as it was for the normal speakers. Shock, therefore, is a greater emotional disturbance in the average stammerer than it is in the average normal speaker. An examination of the individual reactions shows clearly that the greater the vasoconstriction is, the slower is the recovery in practically every case. The relation between the magnitude of the fall and the rapidity of the fall is not so definite when we study the individual curves of normal speakers and stammerers. In about half of the subjects, the time of fall is about the same for both large and small falls; in others it is roughly proportional to the fall, in others it is roughly inversely proportional to the fall, and in still others there is no relationship whatsoever.

Table 6 shows the vasomotor changes of the eight stammerers listed in table 5 during stammering. For these eight stammerers as a class, the average drop was 20 per cent greater in shock than in stammering and the average time of recovery was 2 per cent longer than in stammering. For stammerers as a class, therefore, stammering is almost as great an emotional disturbance as shock. If the curves of shock caused by the shots are disregarded from the results it will be seen that stammering produces a greater emotional disturbance than shock, for the average fall is exactly the same and the time of recovery is 16 per cent longer in stammering than in shock. It will be noticed that the subjects are arranged in both table 5 and table 6 in the order of decreasing magnitude of the fall, and that the subjects do not occupy the same places in the two tables. Thus Bo, who was last in table 5, is practically first in table 6. Web, who occupied first place in table 5, has the last place in table 6. With the exception of Fa, who reacted but once to a single stimulus and who can therefore hardly be assigned to a relative postion in table 5, the other subjects occupied about the same places in the two tables. It is clear that the subjects are not in reverse order in the two tables. It simply happened that Web was the slightest stammerer of the eight and was greatly affected by shock, and that Bo, a senior medical student, was one of the severest stammerers of the eight, and was little affected by shock. The correlation between table 5 and table 6 is high, being +0.919 for the "Falls" and +0.897 for the "T's," if Fa is excluded.

stammer severely; if he reads the same passage a minute later after this constriction has ceased, he does not stammer. If a normal speaker is about to speak a word when a shrill whistle is blown, the sound of that word is suddenly lost and he is unable to say it until peripheral vasoconstriction has ceased; that is, he becomes a temporary stammerer. The typical stammering curve remains low throughout the interval of stammering, with occasional temporary increases and decreases in the low volume. When stammerers are partially cured, the amount of peripheral vasoconstriction decreases and the time of recovery decreases. When the stammerer is cured, his record shows no drop unless it has the same brief attention drop that regularly accompanies other shifts of his attention. If stammering begins with an attention drop as normal speech does in some subjects, the stammering curve will remain low throughout the stammering interval while the normal speech curve will begin at once to return to normal and will sometimes rise above normal as shown in figure 9. Subjects who give an attention drop when they commence to read or speak are apt to give a smaller one when they stop reading or speaking. This delays the recovery in both stammering and normal speech; it is more noticeable on the normal speech curve because the curve is already low on the stammering plethysmograms. Less than half of the subjects show a decided attention drop; those who give it for reading also give it for every shift of attention; hence it can readily be distinguished as an attention drop.

Most of the stammerers who served as subjects had no fear of stammering in my presence and hence could not imagine themselves in embarrassing surroundings. Those who were able to imagine themselves in tight places were told that they would be called upon to introduce two persons or to buy a prescribed article at a store in a given number of seconds. During this period of suspense, Web averaged a drop of 11 mm. in an average of 14 seconds and Wi averaged a drop of 19 mm. in an average of 41 seconds. This shows that the fear of stammering causes marked vasoconstriction even when a stammerer is making no effort to speak.

It is useless for a stammerer to attempt to speak without hesitancy until peripheral vasoconstriction has ceased; hence he must wait at least twenty seconds before he commences to speak and should wait nearly a minute if he is a severe stammerer.

It will be seen that my results do not agree with Fletcher's. Fletcher found that in 90 per cent of his records a rise in the plethysmogram

began immediately after the initial attention drop and usually lasted until the end of the stammering period; I found that, with very few exceptions, the curve remained low from this attention drop to the end of the stammering period. Fletcher has published only a portion of two curves; the rise is not conclusive in either. In his plate D, the writing point which traced the pneumogram kept interfering with the writing point which traced the plethysmogram throughout the rise, lifting it forcibly in several places. In his plate E, there is no normal reference line to go by; the curve is cut from the middle of a period of stammering, and no normal curve is shown either before or after the subject stammered. The curve looks like some of mine that were traced immediately after the subject placed his finger in the plethysmograph when the air in the tubing was still warming enough to expand and cause the recording needle to rise steadily; I suspect that either the room or the air in Fletcher's connecting tubes was warming, causing the air in the tubes to expand and raise the writing point, and that the same rise or a greater one would have occurred during reverie. I frequently found gradual rises due to slow movements in the finger after attention drops, especially in shock; these gradual movement rises were always much steeper than the general rise going on at the same time, and ended in a gradual drop followed by a gradual return to the normal level in approximately that straight line, which, if produced, would pass through the point where the movement rise first began. If these gradual movement rises, due to involuntary, slow tightening or relaxing of the finger lasted for several seconds, it is reasonable to suppose that they would last longer in the arm and that Fletcher may have mistaken them for vasodilatation when he happened to stop the kymograph before the movement ceased and the curve began to fall. I used the Lehmann and the finger plethysmographs simultaneously on one subject and found that when the finger plethysmograph's recorder showed vasoconstriction, arm movements within the Lehmann plethysmograph often caused its recorder to register vasodilatation; the abrupt nature of these rises showed that they must be due to arm movement. All stammerers continually move their arms while stammering but seldom realize the minor movements. These movements affect the Lehmann plethysmograph much more than they affect the finger plethysmograph; it was for this reason that I discarded the former in favor of the latter. I think Fletcher expected a rise and was therefore prejudiced when tabulating his results. He may have discarded curves in which there was a fall, believing this fall was caused

by arm movements. Both Fletcher and I found that the distortions of the plethysmograms are correlated with the severity of the stammering; I found, however, that the amount of the general fall, rather than of the general rise, was correlated with the severity of the stammering.

My results also confirm Fletcher's statement that the curve showed no positive changes, and showed in certain cases a slight decline when the subjects were able to imagine themselves in situations where they would be likely to stammer; my subjects who not only imagined themselves in such situations, but actually lived over again such occasions, gave a marked drop (vasoconstriction), whereas those who were unable to live over again the embarrassing experience gave neither a rise nor a fall. The same vasomotor changes are to be expected whether a stammerer actually stammers or whether he lives over again a situation in which he stammered severely; my results showed vasoconstriction (never vasodilatation) in both cases; Fletcher looked for vasodilatation in both and was surprised to find vasoconstriction or else no vasomotor change when the stammerer imagined himself in a situation where he would be apt to stammer. As such a situation would be apt to be more free from arm movements than would a similar attack of stammering, I think that these results of Fletcher's are correct and that the stammering results are misinterpreted.

TABLE 7

Vasomotor changes in normal speakers accompanying shock brought on by short stimuli, long stimuli and compound stimuli

KIND OF STIMULI	NUMBER OF SUBJECTS	NUMBER OF STIMULI	FALL IN MILLIMETERS			t IN SECONDS			T IN SECONDS		
			Maximum	Average	Minimum	Maximum	Average	Minimum	Maximum	Average	Minimum
Short	7	67	26	10	1	70	18	2	100	39	7
Long	7	43	43	16	3	196	54	5	300	74	11
Compound	5	7	36	19	10	142	73	19	120	97	28

Table 7 shows the comparative vasomotor changes in shock produced by short stimuli, long stimuli and compound stimuli. All curves are those of seven normal speakers. Short stimuli include those listed in table 3. Long stimuli include reading silently several paragraphs from a very exciting novel or from a very horrible description such as *Murders in the Rue Morgue;* taking an unpleasant, unknown object, such as an artificial snake, out of a cloth bag; seeing material for a

TABLE 8

Vasomotor changes accompanying normal speech

SUBJECT	NUMBER OF PERIODS	AVERAGE LENGTH OF PERIODS	NUMBER WITH NO VASOMOTOR CHANGE	NUMBER WITH UNQUESTIONABLE RISES	NUMBER WITH INITIAL ATTENTION DROP	NUMBER WITH FINAL ATTENTION DROP	INITIAL SHIFT OF ATTENTION						READING AND SPEAKING									FINAL SHIFT OF ATTENTION								
							Fall			t			Rise			t			T			Fall			t			T		
							Maximum	Average	Minimum	Maximum	Average	Minimum	Maximum	Average	Minimum	Maximum	Average	Minimum	Maximum	Average	Minimum	Maximum	Average	Minimum	Maximum	Average	Minimum	Maximum	Average	Minimum
K..........	20	69	9	10	2	1	3	2.5	2	22	17	12	11	3.8	2	108	55	25	68	33	17	3	3	3	16	16	16	27	27	27
E..........	4	57	0	4	0	0	0	0	0				10	7.0	3	60	53	38	48	36	22	0	0	0						
B..........	5	74	3	1	1	2	4	4.0	4	19	19	19	4	4.0	4	64	64	64	67	67	67	4	3	2	16	13	10	75	64	53
C..........	7	69	2	3	2	0	2	2.0	2	20	20	20	2	2.0	2	120	77	52	76	66	22	0	0	0						
G..........	3	63	0	3	0	1	0	0	0				7	5.7	5	43	38	34	60	36	17	7	7	7	23	23	23	45	45	45
L..........	7	119	2	1	4	3	15	13.0	11	95	37	12	4	4.0	4	110	110	110	100	100	100	19	11	6	12	7	4	44	40	35
M..........	8	58	3	2	0	3	0	0	0				3	2.5	2	23	18	13	24	22	20	10	6	4	14	10	4	52	35	13
N..........	3	66	1	0	2	0	4	3.0	2	13	8	5	0	0	0							0	0	0						
Q..........	5	95	0	0	5	0	7	4.8	3	60	35	15	0	0	0							0	0	0						
9	*62*	75	*20*	*24*	*16*	*10*	6	6.0	4	38	27	14	6	4.2	3	75	55	48	63	41	38	9	6.6	4	16	12	11	49	43	35

bonfire placed and being told to light it at a signal; and watching a mechanical frog set to go off several seconds after being placed. Compound stimuli consist of several short stimuli given simultaneously or in quick succession. It is evident that "t" will be lengthened in long stimuli and in compound stimuli given at intervals of a few seconds and that "T" will be lengthened when the time of maximum fall occurs near the beginning of a long stimulus or after the first part of a compound stimulus. This table shows that the maximum fall, "t" and "T" are greatest for compound stimuli and least for short stimuli. The maximum fall is 60 per cent greater for long stimuli than for short stimuli, and 90 per cent greater for compound stimuli than for short stimuli.

To ascertain whether the vasoconstriction which invariably accompanied stammering was due in part to reading or speaking per se, I had nine normal speakers read aloud and speak under the same conditions as the stammerers.

Table 8 shows the vasomotor changes of these nine normal speakers while they were reading aloud or speaking. These subjects read short stories which were free from excitement and gruesome descriptions so that emotions could not influence the vasomotor changes accompanying reading, and usually read in periods of from one to two minutes in order to give the vasoconstriction which sometimes accompanied the shift of attention to reading, time to eliminate itself half a minute or more before the end of the reading period.

"t" for "reading" was measured from the instant the subject began to read or speak to the moment when the maximum rise occurred on the plethysmogram. "t" for "final shift of attention" was measured from the end of the reading or speaking period to the time when the maximum fall occurred on the plethysmogram. "T" for "reading" included the final attention drop if there was one. The figures in the bottom line of this table denote the averages for their respective columns; the figures in italics, however, denote the totals for their respective columns.

Table 8 shows that there was no vasomotor change in 32 per cent of the reading and speaking periods; that there was an average rise of 4.2 mm. in 39 per cent of these periods; that there was an average attention drop of 6 mm. when the subject began to read or speak in 26 per cent of these periods; and that there was an average attention drop of 6.6 mm. when the subject stopped reading or speaking in 16 per cent of these periods. In addition to the twenty periods in which

there was no vasomotor change, there were three periods in which the only vasomotor change was an attention drop at the end of the reading or speaking period; hence the number of periods in which there was no vasomotor change practically equalled those in which there was vasodilatation. It is hard to classify under vasoconstriction, vasodilatation, and no vasomotor change, the periods in which there was an initial attention drop. If the maximum vasoconstriction occurred near the beginning of the period and the needle returned to its normal level as quickly as it did in the final attention drop for the same subject, it seemed logical to classify that period under either vasodilatation or no change; otherwise a period seemed to belong under vasoconstriction. As the needle usually returned to normal more quickly than it did in the final attention drop in the periods classified under vasodilatation and no change, and as it might have risen considerably had it not had to regain what was lost during the initial attention drop, I classified all such periods under vasodilatation. This classification gave 5 vasodilatations and 11 vasoconstrictions, which, added to the 24 unquestionable vasodilatations and the single unquestionable vasoconstriction, gave 71 per cent vasodilatations and 29 per cent vasoconstrictions where there was a vasomotor change. This agrees with the results obtained by Doctor Anderson, who found that mental work was accompanied by vasodilatation in 75 per cent of the periods of mental work, and that this proportion for reading was less than that for most other kinds of mental work. Vasodilatation predominated in some subjects, no vasomotor change in others. Attention drops occurred relatively frequently in some subjects and seldom if at all in others. Vasodilatation followed all of the initial attention drops, but a rise above normal occurred in but two periods; the needle returned to normal at or before the end of the reading or speaking period in about half of the periods. The initial attention drop was but two-fifths of that accompanying stammering and was reached twice as quickly in periods averaging approximately the same length, showing that this vasoconstriction was usually due to the shift of attention rather than to the reading. The maximum rise accompanying normal speech occurred at practically the same instant as the maximum fall occurred during stammering, i.e., 55 versus 53 seconds after the beginning of the period. The time of recovery averaged four-fifths of that in stammering, 41 versus 51 seconds, and was measured from the end of "t," which usually occurred several seconds before the end of the period, and not from the end of the period as in stammering.

It will be seen, therefore, that the vasoconstriction which always accompanied stammering was not caused by reading or speaking per se, but by stammering.

I watched the writing lever of the piston recorder whenever I gave a stimulus and noticed there was a latent period of a few seconds before it began to drop. I ran the kymograph at maximum speed for a few curves with a number of subjects to determine more accurately the length of this latent period. I found that this ranged from 1.6 second to 4.0 seconds, and that it averaged 3.4 seconds; this corresponds very closely with the latent period for smooth muscle. The average varied slightly in different subjects. The latent period was about the same in some subjects whether the stimulus was intense or slight; in other subjects it varied by as much as 2 seconds. The fall always began at the same point whether there was a temporary rise or not, and this rise never amounted to more than 2 mm. unless there was a violent movement of the finger. There was more apt to be a temporary rise when the subject jumped or when the stimulus was very intense. I feel sure this temporary rise was due to movement in the finger for it occurred too soon to be caused by vasodilatation. An unexpected stimulus causes one to tighten up more or less even if one does not actually jump; this tightening up is instantaneous but one does not relax at once. The jump itself, if there is one, does not last more than a fraction of a second and is registered as a vertical line on the curve; but there is a gradual readjustment after the jump which lasts for a few seconds. This lasts longer in the arm than in a single finger and explains the same temporary rise mentioned by so many other experimenters. Being convinced that this rise is due to movement and not to any vasomotor change, I have classed it under the general heading of fall, indicating vasoconstriction.

To determine whether the thought process was most impeded at the time when the stimulus was given or "t" seconds later when vasoconstriction in the periphery was at its maximum, I tied around the wrist of each subject's right hand a cord connected through pulleys with a writing lever which registered directly below the plethysmogram. I ruled three columns about one inch apart on a piece of paper and told the subject to add thirteen to the last number in the left column, then seventeen to the last number in the middle column, then twenty-one to the last number in the right column, then thirteen again, and so on; whenever his arm moved to the next column, it displaced the writing needle vertically. The horizontal lines on the record between

two vertical lines measure the number of seconds it took the subject to perform each addition. I gave several short, unexpected stimuli at intervals while the subject was performing this triple addition. A comparison of the plethysmogram with the curve below it showed that the abnormally slow solutions occurred when vasoconstriction was at its maximum, not when the stimulus was given.

SUMMARY

1. Shock and stammering are accompanied in every case by marked vasoconstriction. In a few cases this is preceded by a short, inconspicuous rise in the plethysmogram that is probably caused by arm movement.

2. Vasoconstriction does not begin until about three seconds after the stimulus is given.

3. Long stimuli are accompanied by greater vasoconstriction and slower recovery than are short stimuli.

4. Compound stimuli are accompanied by greater vasoconstriction and slower recovery than are either long or short single stimuli.

5. The more intense the stimulus and the more unexpected the stimulus, the greater is the vasoconstriction, the more rapid the vasoconstriction and the slower the recovery.

6. All the tables show marked individual differences even on the same record.

7. The greater the vasoconstriction, the more is verbal imagery impaired.

8. Those subjects who experience the greatest vasoconstriction during shock and stammering also require the longest time for recovery.

9. Marked peripheral vasoconstriction takes place more rapidly than does slight vasoconstriction.

10. Stammerers experience greater and more rapid vasoconstriction and slower recovery in shock than do normal speakers.

11. Stammerers as a class experience slightly greater vasoconstriction in shock than in stammering. The time of recovery is practically the same in both stammering and in shock. Those subjects in whom the fear of stammering is pronounced, however, experience greater vasoconstriction and slower recovery in stammering than in shock.

12. Vasoconstriction continues throughout the stammering interval; if any vasomotor change accompanies normal speech it is vasodilatation in a large majority of periods.

13. Fear of stammering with no attempt at speaking produces vasoconstriction in the periphery as does actual stammering.

14. Stammerers cannot speak without hesitancy during peripheral vasoconstriction.

15. All stammerers breathe abnormally while stammering. Every stammerer has a characteristic form of breathing while stammering.

THEORETICAL CONCLUSIONS

Stammering and shock are induced emotional disturbances accompanied by the same vasomotor changes. My experiments show that the vasomotor changes in stammerers as a class are of about the same magnitude and in the same direction in both stammering and shock. Unusually intense shock, such as the unexpected report of a pistol, is accompanied by greater vasomotor changes in the periphery than is stammering. Severe stammering, on the other hand, is accompanied in most subjects by greater vasoconstriction in the finger than is shock produced by any stimulus but the pistol. The time of recovery is about the same for stammerers as a class both in stammering and in shock, but is greater for most individuals in stammering than it is in shock produced by stimuli other than the pistol.

The experiments of Mosso and Shepard on trephined subjects reported in an earlier part of this monograph show that shock is always accompanied by vasoconstriction in the periphery and by vasodilatation in the brain. As my results for the periphery confirm those of these experimenters, and as I found approximately the same vasoconstriction in the periphery for stammering as I found for shock, there is every reason to suppose that stammering, like shock, is accompanied by vasodilatation in the brain and hence by cerebral congestion. I have been unable to find a trephined stammerer; as soon as I am able to locate a wounded stammering soldier who has been trephined, I hope to induce him to serve as a subject so that I can prove this supposition that stammering, like shock, is accompanied by cerebral congestion.

It is a well-known fact that fright, which is accompanied by cerebral congestion, may cause temporary partial paralysis of the speech mechanism and of other parts of the body. Persons lose their heads in panics and do inappropriate acts for which they censure themselves later.

Cerebral congestion affects the speech more easily than it affects any other human activity. Many persons have been struck dumb in intense fright. Many normal speakers repeat words or even stammer when greatly excited, and talk incoherently when mentally tired or confused; stammerers experience the greatest difficulty under these same conditions. Several of my normal subjects whose introspection is reliable reported that the words they were about to speak suddenly left their minds when the whistle blew and did not return for several seconds. Some of them still visualized the words and their minds seemed otherwise to work normally, yet the *sounds* of the words left them; hence they were unable to say the words. When vasoconstriction in their fingers ceased, the sound of the words reappeared and they were able to speak them. The same phenomenon occurs in stammering. The fear of stammering and the excessive effort put forth in forcing out difficult words cause cerebral congestion which causes the auditory images of the words the stammerer is about to speak to disappear as he attempts to utter them, just as the whistle banished the sound of the word the normal speaker wished to use and made of him a temporary stammerer. My plethysmograms show that one stammers only when there is vasoconstriction in the periphery and hence, as I have assumed, cerebral congestion. As the stammerer overcomes the impediment in his speech this peripheral vasoconstriction diminishes, and ceases when he is cured.

We see, then, that the fear of stammering and the forcing out of hard words which accompanies stammering cause vasoconstriction in the periphery and cerebral congestion. The latter blurs verbal imagery, especially auditory verbal imagery, and makes it impossible for the stammerer for the time being to recall a part or the whole of the word he wishes to speak at the moment he has to say it. As vowels are governed primarily by auditory imagery they are the stammerers' bugbears. The stammerer prolongs continuous consonants for seconds and repeats closed consonants over and over until able to recall the vowel that follows. The more the vowel is inhibited, the harder he forces out the preceding consonant, the more he thereby increases cerebral congestion, the more impossible it becomes to recall the vowel, and the worse he stammers. Many stammerers are thus led to believe that their trouble lies with the consonants they thus overdo and, by concentrating their mind on trying to force out these consonants, they keep the mind from recalling the vowel which fails to come promptly, and thus increase their stammering.

I have assumed, temporarily, that stammerers have normal verbal imagery when they are not actually stammering. I am conducting at the present time a comparative study of verbal imagery in stammerers and normal speakers which will either confirm or overthrow this assumption. This study will be published in a few months.

My experiments confirm Dr. C. S. Bluemel's theory that stammering is caused by transient auditory amnesia in the auditory speech center brought on by cerebral congestion.

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EXPLANATION OF FIGURES

Unless otherwise stated, the top line in figures 3 to 12 is the time line, the second line is the stimulus line, the third line is the thoracic pneumogram, and the bottom line is the plethysmogram.

Each notch in the time line in figure 3 represents two seconds. As the kymograph ran at approximately the same speed on all of these curves but those shown on figures 11 and 12, the time between any two points in figures 4 to 10 inclusive can be obtained approximately by measuring the distance between the two points and counting the number of notches in a like distance on figure 3 and multiplying by 2. (Allowance must be made for different reductions in reproduction). The exact time is recorded between two notches on the time line in all records, but all or part of this is sometimes missing from the part reproduced.

A notch on the stimulus line indicates when a stimulus was given or when a subject began or stopped reading or talking.

The top of the pneumogram indicates full lungs and the bottom empty lungs, unless otherwise stated.

The top of the plethysmogram indicates vasodilatation, the bottom vasoconstriction.

The following curves are all typical reactions; I have avoided reproducing extremes or abnormal curves. If any reader wishes to see my other curves, he is invited to examine those in my album at the Boston Stammerers' Institute.

Figure 3 shows a typical reaction of a normal speaker to shock. This curve was made by subject D on February 26, 1918. The tambour that recorded the thoracic breathing was inverted in this record so that the top of the pneumogram represents empty lungs and the bottom full lungs. At the notch marked *22*, I placed a mechanical frog near the subject. At the next notch, about a quarter of an inch to the right, this frog jumped 4 feet into the air without the slightest warning. At *23*, the subject read silently a dull book until a point well beyond the part reproduced; this occupied the subject's attention and thus prevented arm movements. Note the fall of the plethysmogram when the frog was placed, the temporary rise when the frog jumped (due here to movement when the subject jumped), the decided drop following this brief rise, and the much slower return to normal. Other curves show that recovery is the same whether the subject is in a state of reverie or is reading silently. The straight line tangent to the plethysmogram at its two ends is the path that would have been traced had no stimulus been given.

Figure 4 shows two typical reactions of a stammerer to shock. This curve was made by subject Wi on August 1, 1918, a few minutes before the curve reproduced in figure 5. At *W*, a shrill whistle was blown without warning and at *Y*, the operator gave a loud, sudden yell. The time line shows that an interval of a minute and a half elapsed from the notch to the right of *W* to the notch to the left of *Y*. The straight lines on the right hand half of the plethysmogram show how "Fall," "t" and "T" are measured; the line tangent to the plethysmogram at its two ends shows the path that would have been traced had no stimulus been given, the vertical line represents the maximum fall and cuts the stimulus line at a point "t" seconds from the notch where the yell was given and "T" seconds from the point of tangency at the extreme right of the reproduction. Note that the drop for the whistle is much greater than that for the yell.

Figure 5 is a typical stammering curve made by subject Wi a few minutes after the curve shown in figure 4. At the left hand stimulus notch, this stammerer was told that he would be called upon to introduce two persons in fifteen seconds. At the longer notch, about half an inch to the right, he began the introduction, stammering considerably; at the next notch, a quarter of an inch to the right, he finished speaking. The long notch in the time line has no significance. Note that the curve fell rapidly soon after the subject was told that he would be asked to introduce two persons and remained low throughout this period of warning, that it fell nearly as much below this point soon after the subject began to stammer and remained low throughout the stammering interval and for a few seconds after stammering ceased, and that it returned much more gradually to normal. The room was cooling down somewhat, hence the curve fell gradually throughout the record.

Figure 6 is a typical curve of a long period of stammering made by Wi on the same day as that reproduced in figure 5. At the notch below *17*, the subject was told to check a trunk to Portland, Maine, in forty-five seconds. At *18*, he began to speak to a baggage master whom he saw clearly in his imagination and I kept him stammering to the first long notch on the time line by asking him unexpected questions about his trunk. The short notches close together on the time line indicate when he was stammering most severely. The two long notches at the right with *1 m* between them indicate a period of one minute. The straight

line below the pneumogram represents the path that would have been traced if the subject had not been disturbed. Note that the curve fell rapidly soon after the stammerer was told he would be called upon to check his trunk in forty-five seconds and remained low throughout both this interval and the stammering which lasted about a hundred seconds. There was a gradual arm movement during a part of the period of recovery.

Figure 7 is a typical curve of a stammerer reading without being told in advance that he is to be called upon to read. It was made by Bo on April 23, 1918. At the notch below *3*, he began to read and at *4* he stopped reading. Note the steady fall throughout the reading period and the steady but more gradual rise to normal, beginning with a slight finger movement at the close of the reading period. There is another gradual finger movement near the end of the period of recovery. The pneumogram is inverted in this record.

Figure 8 is a typical curve made by a cured stammerer reading without hesitancy. It was made by Bo on November 26, 1918, after he had been taught how to read without hesitancy. The subject read without stammering for one minute between the single and double notches on the stimulus line. There is a distinct breathing wave in the plethysmogram (due to movement). There was a gradual movement of the finger at the end of the reading period. The curve rose very slowly throughout the record because the air within the tube was warming up and expanding. The breathing appears very much deeper on this record than on the others because a much more sensitive pneumograph was used; this pneumogram is not inverted. Note that the plethysmogram did not fall as it did in figure 7, when the same subject stammered severely while reading a similar passage from the same book.

Figure 9 is a typical curve of a normal speaker reading, showing an attention drop. At *5*, the subject began to read; at *6*, he stopped reading. The time notches above *5* and *6* indicate that he read for exactly one minute. After the attention drop at the beginning of the reading, there was a slow rise to normal with occasional gradual arm movements. The curve rose gradually throughout this experiment, as the air in the tube was growing warmer.

Figure 10 is a typical curve of a normal speaker reading, showing no attention drop. He read for about one minute, beginning at the left hand notch and stopping at *6*. The curve is as regular as that in reverie preceding and following it. The subject's finger moved slightly while he was reading. The engraver omitted the time line which happened not to be needed.

Figure 11 is a typical shock curve traced when the kymograph was running at maximum speed to show the latent period of vasoconstriction. It was made on November 18, 1918, by the same normal speaker who made the curve reproduced in figure 9. At the extreme left hand notch on the stimulus line, I yelled suddenly. The time notches in the right hand half of the record were made at intervals of ten seconds. Note the slight rise due to arm movement the instant I yelled, the distance from this point to the point where vasoconstriction began, the rapid vasoconstriction and the gradual arm movement at the beginning of the slow rise to normal which is reached at the very right hand end of the reproduction. Reference lines made at the right hand end of all four curves show that the four recording needles were working nearly in a straight line. The breathing appeared more shallow than that in the other records because the less sensitive Verdin pneumograph was used.

Figure 12 contains a number of pneumograms taken at maximum speed. The left hand half of the one at the top is that of a normal speaker reading. The right hand half is a typical pneumogram of a cured stammerer reading without hesitancy. The pneumogram just below is that of a stammerer who habitually spoke on empty lungs. The middle one is that of a stammerer who habitually spoke on full lungs. The two near the bottom are those of stammerers who spoke now on full lungs and now on empty lungs; note their irregularity.

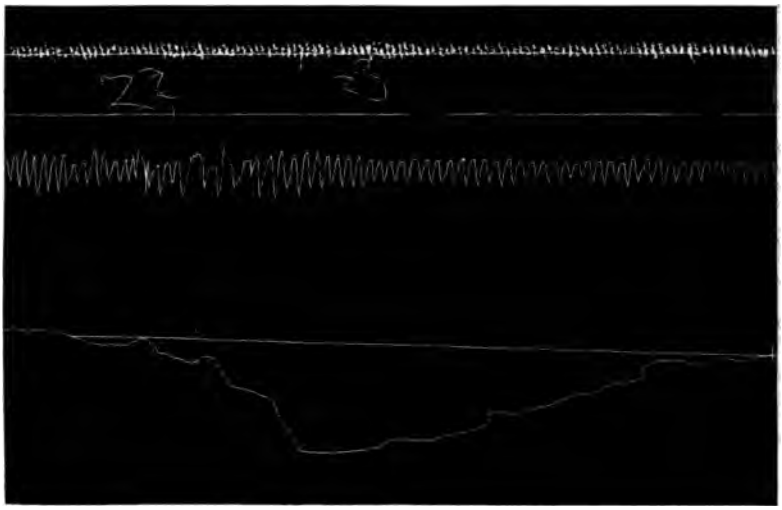

Fig. 3. Three-fourths natural size

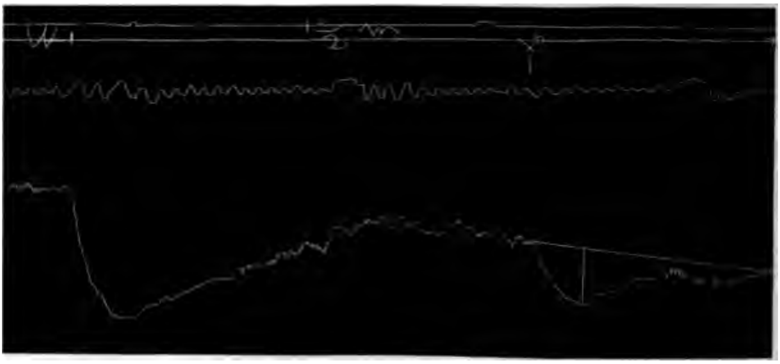

Fig. 4. Three-fifths natural size

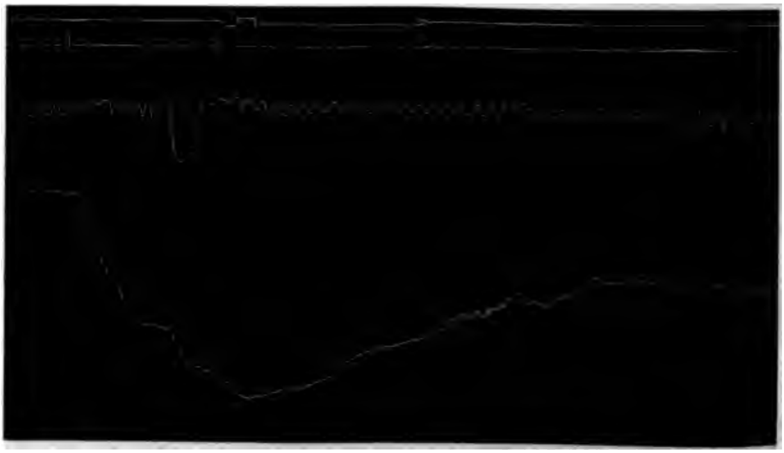

Fig. 5. Two-thirds natural size

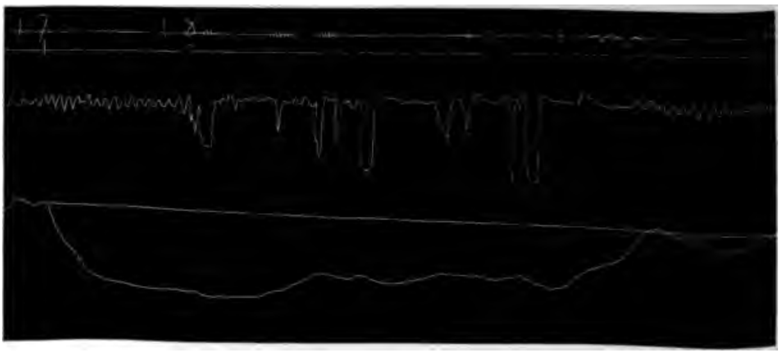

Fig. 6. Three-fifths natural size

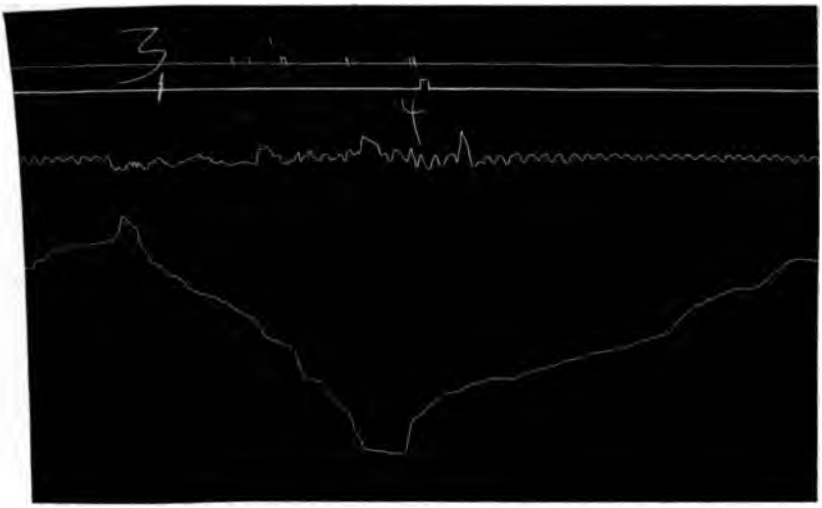

Fig. 7. Four-fifths natural size

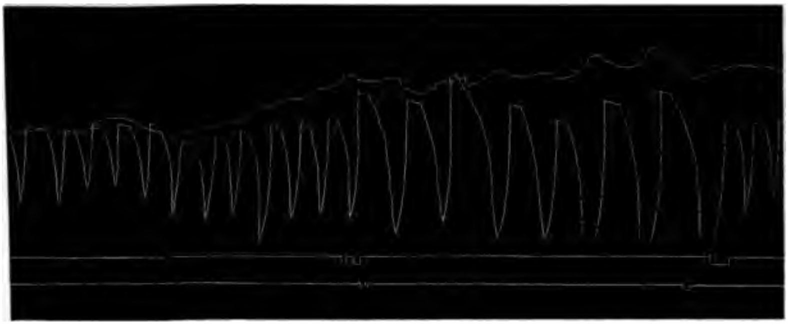

Fig. 8. Three-fifths natural size

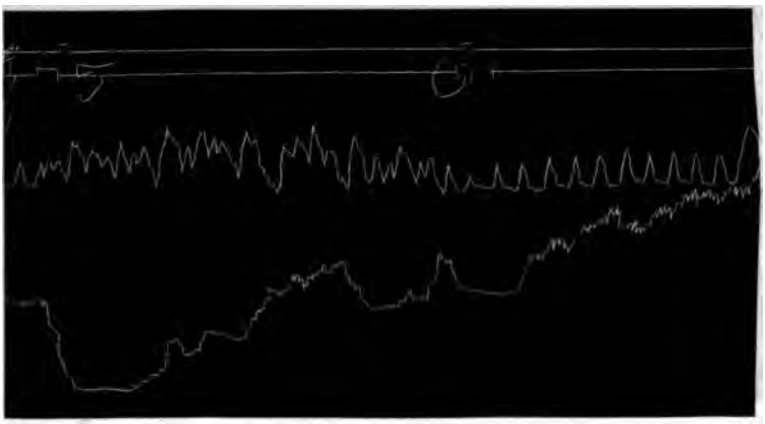

Fig. 9. Three-fourths natural size

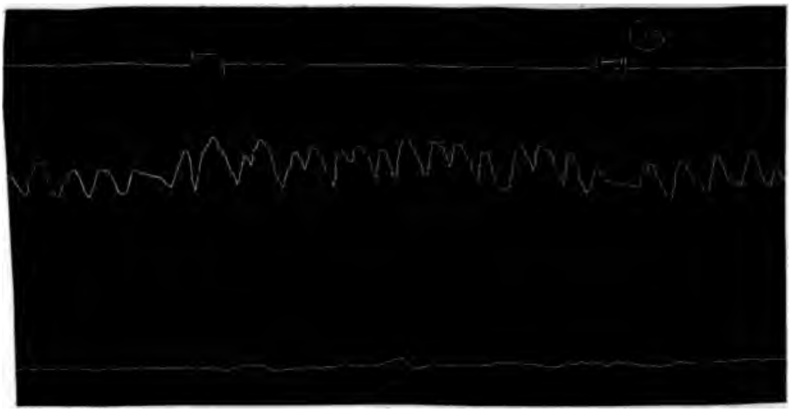

Fig. 10. Three-fourths natural size

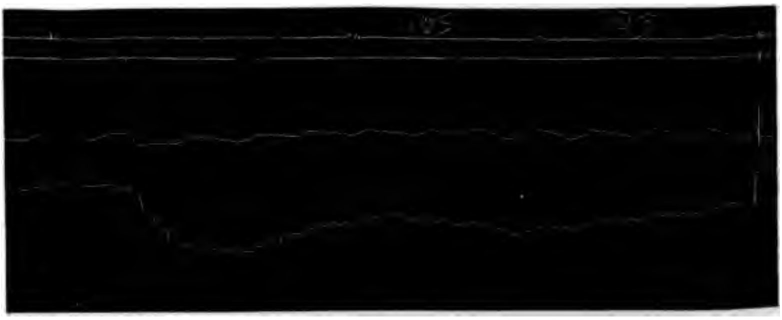

Fig. 11. One-half natural size

Fig. 12. One-third natural size

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A PLETHYSMOGRAPHIC STUDY OF SHOCK AND STAMMERING IN A TREPHINED STAMMERER

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The experimental work here reported was done in January, 1920. The purpose of the experiment was to test the organic reactions accompanying stammering, with special reference to Dr. C. S. Bluemel's cerebral congestion theory of stammering, and to determine whether stammering, like shock, is accompanied by congestion in the brain, and hence by increased intracranial pressure, as was conjectured by me in the theoretical conclusions to "A plethysmographic study of shock and stammering" published in the April, 1919, number of this Journal.

Subject. The subject who took part in this experiment was born in Telsburg, Norway, November 14, 1875, and came to America in 1893. He went to school for nine years in Norway. He speaks Norwegian, Swedish and Danish as well as English, and used to speak Spanish; he stammers equally in all languages.

He is a laborer of low intelligence; he was a sailor for five years, a rigger for ten years, a pipe fitter for four years and a pile driver two years. He attained a total score of 21, rating D, in Group Ex ination Alpha used in the United States Army, corresponding to a mental age of eleven years. His verbal imagery was the le vi of any of the subjects tested in (5): his auditory was 1.03 com with the average, 2.2: his kinesthetic was but 0.13 compared with average, 0.8; and his visual but 0.27 compared with the average, stammerers, 1.2. All types of his non-verbal imagery were vivid ex cept the kinesthetic.

The subject spoke without hesitancy until, at the age of eight, was so terrified on the ice by a boy who impersonated a bear that was rendered speechless for two hours and has stammered ever

[1] The subject's salary and expenses were paid from a grant of $100 fi Committee on Grants of the American Association for the Advancen t o Science.

His stammering was increased at the age of thirty when he was shot by a robber, the bullet splintering the skull bone over the right eye and necessitating the trephine described by Doctor Cobb. The skin of the noticeable dip or hollow covering the trephine was loose and free in its movements and the pulsations were often conspicuous.

As far as can be learned, the subject inherited no tendency to stammer. He sings normally and reads and speaks without hesitancy when alone. He frequently stammers on words beginning with H, L and R and sometimes repeats the first letters of certain words without contortions, but often avoids this stammering by using synonyms for the words he wishes to speak. He is not nervous or excitable, and is not apparently embarrassed or sensitive about his stammering. Pneumograms traced while the kymograph was running at high speed showed that he breathes correctly before he speaks, but continues to speak after his lungs are empty. In short, the physical aspect of his stammering is far more prominent than the mental aspect, and the impediment in his speech, which is of a common type, would not be difficult to correct in a younger man who was willing to work conscientiously.

The subject was examined at the Massachusetts General Hospital by Dr. Stanley Cobb on January 8, 1920. His report follows:

Complaint: Stammering.

Past History: Smallpox at 21. Gonorrhea at 23 and 24. At about this time he also had a hard chancre for which he was treated with pills and inunctions for three months. He does not know of any other sickness except that of August 30, 1905, when he received a gunshot wound in the right forehead which perforated the skull. He was taken to the Long Island College Hospital and operated on. Evidently the bullet was removed and a small trephine hole left open.

Physical Examination: Cranial nerves: Smell normal. Ocular movements normal. No squint or difficulty in convergence. Muscles of mastication ong and equal. The sensation on the face is normal. There is no facial akness. Hearing tests show slight deafness in the left ear. The Rinne test referred to the left. The defect seems to be in the air conduction apparatus. The drum appears normal. Taste is normal. The pulse is regular and slow (see cial report in experiments). No weakness of the sternomastoid or trapezius les. Tongue protrudes in midline. The fundi show normal vessels. In right eye the disk is sharp but in the left the margins are slightly hazy. Visual y is within normal limits. He does not wear glasses.

Reflexes: Biceps, triceps, knee and ankle jerks are all equal and if anything ghtly depressed. The superficial reflexes of the abdomen and scrotum are ghtly more active than usual and equal on the two sides.

Motor system: The muscle groups are strong and symmetrical. There is

no disturbance of gait, no ataxia, asynergia or aphonia. There is a slight fine tremor of the extended fingers. Rhomberg test normal.

Sensory system: There is no disturbance of touch, pain or discrimination on any part of the body.

Endocrin system: Thyroid not enlarged. Bony development normal. The distribution of the hair on the body and face is normal. The testicles are in normal position and well developed.

Sympathetic system: The pupils react to light and accommodation. They are round and equal. Skin reactions are slight. There is no dermographia.

Skull: In the right fronto-temporal region 5 cm. from the midline is a defect in the bone irregularly circular in shape, its widest diameter being 2.5 cm. and its narrowest 2 cm. This pulsates visibly and when the patient leans over it is seen to bulge slightly.

Respiratory system: Lungs are clear. The nose, throat and tonsils are negative.

Cardiovascular system: The heart is not enlarged and the sounds are clear and without murmurs. The peripheral arteries are palpable but not thickened. The pulse is full. Blood pressure 110/80 (see special experiments). The aortic second sound is exaggerated and somewhat louder than the pulmonic second.

Alimentary system: The teeth are in fairly good condition except for three bad roots. The abdomen is level, soft and tympanitic. There are no masses felt.

Blood: No anemia. Wassermann reaction negative.

Mental status: General behavior: Normal, quiet and coöperative.

Stream of talk is slow but pertinent.

Mood: No depression or elation of spirits.

Special preoccupations: No worries, imaginations, delusions or hallucinations.

Orientation: Accurate for time, place and person.

Memory: Accurate for remote and recent events.

General Information: He knows the dates, names of the principal government officials, etc., but shows no interest in the affairs of the country.

Speech: There is a marked stammering, especially when embarrassed by the presence of a stranger. The sticking mainly occurs on the hard consonants. There is no aphasia, apraxia or astereognosis.

Diagnosis: An individual of the mentally dull type, probably not to be classed as a real defective, who has sustained a bullet wound of the right frontal region. There is no evidence of any brain injury.

Apparatus. The apparatus used in this experiment included the same Zimmerman kymograph, Sumner pneumograph, finger plethysmograph, piston recorder and the two electromagnets described in (6), pages 293 to 302, a less sensitive tambour than the one used in my earlier work, a second piston recorder like the first, and two forms of brain plethysmograph.

The first form of brain plethysmograph, which I will call the rubber plethysmograph, consisted of a hard rubber cup having an edge of soft rubber. A metal tube led out through the top side of the cup, and a rubber tube connected this with a syringe and piston recorder as in my earlier experiment. This plethysmograph was held in place by a single bandage passed around the subject's head from front to back. As the pressure of this bandage gave the subject a severe headache, most of the experiments were performed with the second form of brain plethysmograph, consisting of a glass funnel 4.7 cm. in inside diameter, which was cemented to the subject's forehead with collodion and removed with ether. This glass plethysmograph had two other advantages over the rubber one: it was not pressed against the forehead by bandages whose pressure varied with head movements, and an observer could detect sudden pulsations within the trephine which registered on the drum so much like movements, that in the records where the rubber plethysmograph was used I mistook them for movements and called in expert witnesses while the records were being traced to affirm that these abrupt rises were not due to movement in the large majority of cases.

With the apparatus described in (6), page 300, it was found that a rise of 1 mm. on the records always denoted an increase in volume of the brain of 3.0 cu. mm. when the rubber plethysmograph was used, and of 2.5 cu. mm. when the glass plethysmograph was used.

Procedure. The procedure and arrangement of apparatus was practically the same as described in (6), pages 302 to 306, except that the kymograph was run at higher speed to show the pulse. As five tracings were being made at the same time, it was very difficult to readjust one writing needle without throwing another out of adjustment. Being most interested in the brain tracing, I neglected the other writing needles when the one tracing the brain volume needed attention. For this reason, the needle which traced the finger volume and which moved freely only when it just touched the drum was frequently pressed too tightly against the drum to make a true tracing. The vasomotor reactions studied in (6), where this delicate pressure was kept constant, are far more reliable than those in this study.

As the pressure of the needle tracing the finger plethysmograms could not be kept uniform, no attempt was made to measure the finger plethysmograms; these were recorded simply as increases or decreases.

To determine what route the writing needle connected with the brain plethysmograph would have taken had the subject done no mental or

physical work and been given no stimulus, I placed the celluloid triangle along the crests of the highest pulses in the troughs of the Traube-Hering waves of the rest period immediately preceding and of that immediately following the period of work or disturbance. As the Traube-Hering waves were never large in these rest periods, and as the pulses at their troughs were much more uniform than those at their crests, this gave a fairly accurate reference line, though of course measurements made from it were necessarily approximate compared with those made from as accurate a reference line as was determined for the finger plethysmograms in (6). The lowest point on each plethysmogram, like the highest, was always considered to be the crest of a pulse at any phase of a Traube-Hering wave.

About half of the curves had to be discarded because of very slight leaks which developed in the brain plethysmograph when movements made by the subject loosened the bandage of the rubber plethysmograph or weakened some spot in the collodion which held on the glass plethysmograph; the discarded curves confirmed those retained as to rises and falls, but did not admit of accurate measurement. If the brain plethysmogram was reliable, the curve was retained, no matter how imperfect the finger plethysmogram.

The subject was seated in a large Morris chair so inclined that the trephine was nearly horizontal when he laid his head back in the chair; this put the least strain on the collodion which held on the glass plethysmograph.

An observer, seated close to the subject in most of these experiments, pressed a key whenever the subject moved his head, thereby enabling me to determine which abrupt rises in the brain plethysmograms were due to a rapid change in the brain's volume. In experiments arranged to study the effect of movements, it was found that small movements showed little if at all in the records, whereas big or quick movements were noticeable, raising the head or turning it to the right appearing to give a rise, lowering the head or turning it to the left appearing to give a fall. Curves containing head movements after which the w ing needle did not return to normal were discarded.

Table 1 contains a brief summary of my results. The percentages of increases, decreases, no changes and complex reactions are given for both brain and finger volume: + denotes increase in volume; − denotes decrease in volume; 0 denotes no change in volume; +(−) denotes a prolonged increase followed by a short decrease; and so on. Rise in millimeters was measured to the highest point of the brain

plethysmogram from the path its writing needle would have traced had the subject's mind been a blank. This normal path was assumed to pass through the crest of the highest pulse at the trough of each Traube-Hering wave. The column entitled "Increase in height of brain pulse" gives the number of times greater in height the average pulse during the performance of an assigned task was than the normal pulse in the rest periods immediately preceding and immediately following the period of activity. The maximum pulse referred to in the next column is the highest brain pulse traced during the period of activity. t gives the time in seconds from the beginning of an assigned task to the time when the recording needle connected with the brain plethysmograph first attained its maximum rise. T gives the time in seconds from this maximum rise to the time when this recording needle first returned to normal, that is, to the path it would have taken had the subject's mind remained a blank. Or, if the time of maximum rise occurred while the subject was performing an assigned task, T was measured from the instant of completion of the task instead of from the end of t.

Every period of stammering while reading or speaking showed marked increase in brain volume usually accompanied by a greatly increased pulse for at least part of the period (see figs. 4, 5 and 6). Six periods of reading, averaging 77 seconds in length, gave rises of from 23 to 88 mm., averaging 49 mm. The maximum height of pulse registered during each reading period ranged from 1.7 to 5.0 times the normal size of pulse before and after the period of stammering, averaging 3.3 times the normal pulse. Twelve periods of talking, averaging 95 seconds in length, gave rises of from 32 to 70 mm., averaging 53 mm., and maximum pulse of from 1.4 to 7.0 times the size of the normal pulse, averag- 4.2 times the normal pulse. Thus there appeared to be slightly gr er cerebral congestion during talking than during reading. The for these reading and talking periods, taken together, ranged from 3 to 88 mm., averaging 52 mm.; and the maximum pulse for the same eriods ranged from 1.4 to 7.0 times the size of the normal pulse, aver- ng 3.9 times the normal pulse.

The maximum rise occurred on the average near the middle of the period in both reading and speaking. The brain volume and pulse became normal in from 20 to 46 seconds, averaging 33 seconds, after the end of the reading periods, and in from 0 to 82 seconds, averaging 37 seconds, after the end of the talking periods.

TABLE 1

Vasomotor changes accompanying various physical and mental states

WHAT SUBJECT WAS DOING	NUMBER OF CURVES	FINGER VOLUME	BRAIN VOLUME	BRAIN VOLUME RISE IN MILLIMETERS			INCREASE IN HEIGHT OF BRAIN PULSE			NUMBER OF TIMES MAXIMUM PULSE EXCEEDED NORMAL			*t*	*T*
				Maximum	Average	Minimum	Maximum	Average	Minimum	Maximum	Average	Minimum		
		per cent	*per cent*											
Stammering	18	0 62 + 12 − 6 +− 19	+	23	52	88	Irregular			1.4	3.9	7.0	Middle	35
Reading aloud with no one in the room	1	+	+	53	53	53	1.8	1.8	1.8	2.5	2.5	2.5	Middle	18
Reading normally	6	0 67 + 33	+ 66 −+−17 0 17	−6	9	20	1.5	?	4.0	1.0	3.3	8.0	End	32
Silent reading	7	0 57 + 29 +− 14	+ 42 +−+29 0 29	0	14	29	1.0	1.0	1.0	1.2	1.4	2.0	Middle	55
Other mental work	8	+ 50 0 33 +− 17	+ 74 −+ 13 0 13	0	19	40	−?	+?	3.0	1.2	2.5	4.0	End	55
Physical work	6	+	+	10	21	25	[illegible]	[illegible]						

Increased intrathoracic pressure.............	5	+ 20 0 40 − 40	+(−)	52	69	82	+	+	+	1.5	11.3	22.5	Start	36
Clearing throat........	2		+(−)	63	66	70	+	+	+	1.6	1.7	1.8	Start	16
Sniffing...............	2	0	+	58	59	60	2.0	4.0	6.0	3.0	6.5	10.0	End	36
Deep breathing........	5	0 67 − 33	+ 20 − 80	5	−12	−32	−1.7	−2.3	−3.0	1.0	3.3	10.0	End	43
Fear of stammering....	4	0	+	15	28	45	+	+	4.0	2.1	4.5	5.8	?	?
Shock.................	10	0 87 − 13	+ 80 0 20	0	16	38	+	+	+	1.0	3.8	8.3	15	23

Increased intrathoracic pressure	5	+ 20 0 40 – 40	+(–)	52	69	82	+	+	+	1.5	11.3	22.5	Start	36
Clearing throat	2		+(–)	63	66	70	+	+	+	1.6	1.7	1.8	Start	16
Sniffing	2	0	+	58	59	60	2.0	4.0	6.0	3.0	6.5	10.0	End	36
Deep breathing	5	0 67 – 33	+ 20 – 80	5	–12	–32	–1.7	–2.3	–3.0	1.0	3.3	10.0	End	43
Fear of stammering	4	0	+	15	28	45	+	+	4.0	2.1	4.5	5.8	?	?
Shock	10	0 87 – 13	+ 80 0 20	0	16	38	+	+	+	1.0	3.8	8.3	15	23

The finger plethysmograms gave about an equal number of vasodilatations and vasoconstrictions in both reading and speaking, two +, one −, one + −, one − +, one − + −, and ten no change; finger movements made it impossible to determine the other reactions.

It is important to determine what part of the increased intracranial pressure is due to stammering and what is due to the ordinary mental and physical work of reading or speaking. The subject read aloud in the room by himself without hesitancy; but there were the usual disturbances in breathing and the employment of superfluous effort which are usual in stammering whether reading alone or in public; and the single record (see fig. 3) in which the results were not injured by movement or by the needles' leaving the drum when no experimenter was in the room gave a maximum rise of 53 mm. in the middle of the 140 second reading period, an average pulse 1.8 times the normal pulse before and after reading, and a maximum pulse 2.5 times the normal, 25 mm. compared with 10 mm. The volume and pulse returned to normal more quickly, however, than after a period of severe stammering, this being accomplished in about 18 seconds. The finger plethysmograms showed marked vasodilatation. This single record indicates that a stammerer may have increased intracranial pressure while reading aloud or speaking whether he is stammering or not, but that this intracranial pressure is highest during periods of severe stammering.

A more satisfactory comparison would be that of the subject reading aloud a given passage and reading aloud another from the same book after he was cured. I was, fortunately, able to teach him in five days to read without hesitancy to any person who came into the room, and believe a comparison of records obtained while he was thus readi normally with the above stammering reading records will be a comparison of reactions to stammering and normal speech. He r aloud in this way in six periods free from movements in periods ave ing 65 seconds in length (see fig. 7). Every time the subject brea he raised his head, causing a distinct rise on the brain plethysm A wave was thus caused in the curve which might be termed an direct breathing curve. This curve was neglected in making r ments of changes in brain volume. One record gave no cha brain volume, one gave −6, + to 0, −6, and four gave rises from 12 to 20 mm., averaging 15 mm. The average pulse during these peri varied greatly and could be determined in only two records, in increasing from 5 to 20 mm. and in the other decreasing from 12 to 8 mm. In two of the records the maximum pulse did not increase at all,

and in another it was as high as 8 times the normal, averaging 3.3 times the normal pulse. The greatest rise occurred in the middle of the period having the greatest average pulse and near the end of the other 5 periods. The time of recovery was 24 seconds in one, 40 seconds in another, and in doubt in 4. The finger plethysmograms showed a rise in two and no change in four of the records. There appears, therefore, to be much greater intracranial pressure in stammering reading than in normal reading, the average rise being 49 mm. compared with 15 mm.

To determine how much of the rise in normal reading was due to the mental work of the reading itself, I had the subject read silently seven passages in periods averaging 46 seconds (see fig. 2). The brain tracing showed no change in two, $+-+$ in two, and $+$ in three; where there was a $+$ in any part of a reaction, it ranged from 12 to 29 mm., averaging 20 mm., and will therefore account for the rise in normal reading aloud. The average pulse was about the same as the normal pulse, and the maximum pulse ranged from 1.2 to 2.0, averaging 1.4 times the normal. Hence the mental work of reading does not account for the increased pulse during normal reading; on the other hand, it will be ·n that the physical work does account for this. The greatest rise occurred near the middle of the reading period and the time of recovery averaged 55 seconds, as it did in the case of other kinds of mental work. The finger plethysmograms showed one $+$, one $+-$, and four no changes.

Other kinds of mental work, including checking additions, multi- ions and divisions (see fig. 2), and counting the number of E's on a page, gave reactions similar to silent reading in eight 1 to 3 minute ·iods. The brain volume remained the same in one, $0+$ in another, $(+)(-)+$ in another, $-+$ in another, and $+$ in four, the rise varying from 0 to 40 mm. and averaging 19 mm. The average pulse during periods of mental work was the same as the normal in four tracings, w lower in one, and was higher in three, as much as 3 times higher in case; the maximum pulse was from 1.2 to 4.0, averaging 2.5 times normal, being greater than that in reading silently. The maximum occurred near the end of the period of mental work, and the time recovery ranged from 15 to 99 seconds, averaging 55 seconds. The finger plethysmograms showed no change in two curves, $+$ in three, $+-$ in one; and two were in doubt.

Periods of physical work on the ergograph used by Anderson and described in (1) page 42, gave six clear cut results (see fig. 1). There was

one fatigue period which lasted 184 seconds; the other test periods lasted one minute each. There was a rise of the brain plethysmogram with increased pulse in five periods, (+)(−)0 with larger pulse in the sixth. The rises ranged from 19 to 25 mm., averaging 21 mm., including the increased pulse which averaged 16 mm., about half of which is above the line drawn through the center of the pulse. The average pulse during physical work was from 2.4 to 8.0 times the normal, averaging 5.4 times the normal, and remained nearly uniform throughout the period of work, the maximum ranging from 1.2 to 1.7 and averaging 1.4 times the average pulse during the period of work. In the case of the long fatigue period the maximum rise occurred near the end; and in the short periods its position varied. The time of recovery ranged from 18 to 72 seconds, averaging 54 seconds. The finger plethysmograms gave + in five of the curves and (−)+ in the other: in this one the brief fall was due, no doubt, to the increased mental activity (to the change of task. Physical work, therefore, is accompanied by slight increase in both brain and finger volume, and by marked inc in brain pulse.

It seemed well, also, to study the brain volume during increased intrathoracic pressure, for this occurs with the muscular spasms which accompany stammering. The subject was asked, therefore, to c his throat and also to bear down as if straining at the stool. In b cases the immediate rise was so great and so abrupt that I thoug it was caused by a movement of the head until I saw through the g plethysmograph that the skin over the trephine flattened out i at these times and that the subject did not move. Both show brief fall in brain volume after a marked rise, then a slow return normal, the high pulse gradually decreasing (see fig. 9). The varied from 52 to 82 mm., averaging 68 mm. The maximum averaged 1.7 times the normal in the clearing of the throat, and 11. times the normal in the bearing down, being 23 times the nor one case, and even higher in discarded records where the piston of the recorder came out of its barrel. The time of recovery averaged 16 seconds for the clearing of the throat, and 36 seconds for the bearing down.

In one record chewing chocolate gave a rise in brain volume of 64 mm., the average pulse during chewing increasing from the normal of 9 to 20, and the maximum pulse to 45. This rise was not due to the pleasant sensation of the chocolate, as there was no appreciable rise during the half-minute he held this on his tongue before chewing; or

to the opening and closing of the mouth per se, as this gave a fall of 7 mm. and a low pulse when the subject opened his mouth every time he inhaled and closed it every time he exhaled. It must have been due to the physical work of chewing, and might be expected in any stammerer who forces words with his jaws.

A comparison of different kinds of breathing was made to learn what effect these had upon intracranial volume. Holding a deep breath caused a fall at first, then a rise until the subject breathed. Twelve seconds after the subject had kept his lungs empty for fourteen conds, a maximum rise of 38 mm. occurred with maximum pulse arly double the normal. Two records of sniffing (see fig. 8) gave s of 58 and 60 mm. with average pulse during the period 6 times t normal in the first and twice the normal in the second. The time of recovery was 40 seconds in the first and 33 seconds in the second. There was no change in finger volume. The actions of clearing the throat and sniffing both tend to fix the diaphragm and thus cause increase of intrathoracic pressure.

Deep breathing, on the other hand, had quite a different effect (see fig. 8). Three periods gave (+)−, one +, and one − in the brain tracing, and one − and two no change in the finger tracing (two were obscured by movement). The single instance where the change in brain volume was + throughout was but 5 mm.; the four decreases ranged from 10 mm. to 32 mm., averaging 16 mm. The average pulse during this deep breathing either remained the same or decreased, the decrease ranging from 1.7 to 3 times as low as that of normal, averaging 1.8 times as low. The greatest decrease occurred at the end of each period.

In one record I had the subject read aloud normally, then breathe deeply, then read normally again, and then read silently, without pausing between these periods. The curve showed little change for these different periods of mental and physical work. The volume remained about 12 mm. above normal throughout the first normal reading period, averaged 10 mm. during the period of deep breathing, r from 10 to 13 mm. during the second period of normal reading, d kept at 12 mm. during the period of silent reading, the pulse being higher for the silent reading period than for the other periods.

Fear of stammering was also compared with shock as in my earlier udy, but in spite of my many efforts to get the subject to live over in an experience in which he feared that he would stammer, I was le to cultivate this emotion only four times. On the most successful

occasion (see fig. 5), I told him that a lady to whom my pupils found it most difficult to speak would enter the room in a few minutes and ask him some questions; she entered at the psychological moment. The four above mentioned brain plethysmograms gave rises of 15, 19, 33, and 45 mm. with increases in pulse, the average pulse increasi 4 times above the normal in the 33 mm. rise in figure 5. No cha finger volume could be detected in any of these curves.

My subject's reactions to shock confirmed those of Shepard, (7) and (8), discounting the fact that my subject was less emotional (see fig. 10). There was no change in brain volume in two of the ten reactions to the quick stimulus of a single loud noise, and an increase in the other eight ranging from 9 to 38 mm., averaging 20 mm. The maximum pulse ranged from no increase to an increase of 8.3 times the normal pulse, averaging 3.8 times the normal pulse. The maximum rise occurred from 5 to 18 seconds after the stimulus (30 seconds in the case of one stimulus whose length was in doubt), averaging 15 seconds. The time of recovery ranged from 14 to 34 seconds, averaging 23 seconds. The finger plethysmograms showed a decrease in one case, no change in six cases, and movements in the other three; this proves my assertion that this subject was far less emotional than t subjects in my earlier experiments (6).

Now and then I noted a decided rise in a rest period and asked my subject to tell me at the end of the experiment whether he was think of something pleasant or unpleasant just then; I found a reason for every such rise. In one case something exciting flashed in his mind for 4 seconds causing a rise of 12 mm. A very pleasant emotion for which he reported he was miles away caused a rise of 23 mm. (see fig. 1) a maximum pulse 13 times the normal; the finger volume also incr l in this case. I was unsuccessful in getting him to cultivate emotic at will, so had to study emotions by retrospective reports in this way.

In one case I had an assistant make a loud noise behind the subject in the middle of a speaking period and found that this made him stammer worse and greatly increased the brain volume; this noise startled the subject so that he moved enough to make a reference line inaccurate, but not enough to spoil the record.

So far as I know, there has been no previous work on a trephin stammerer. A comparison of my results with those of Berger, M Shepard and Weber upon normal speakers, summarized under "b volume" at the bottom of table 1 (6, p. 289), shows that my re ts agree in every case with those of Mosso and Shepard, and agree with

se of Berger and Weber in the reactions to stimuli and to mental l physical work.

ι few readings of the subjects systolic and diastolic blood pressure ·e taken at intervals while the experiments were in progress. The ject's normal blood pressure averaged 110/80. When asked a sud- ι question and put completely off his guard, Doctor Cobb found the ject's blood pressure to be 145/108. Bearing down sent the blood ssure up to 125/104, slight stammering to 122/?, physical work on ergograph to 118/?. Increase in brain volume would seem, there- ɜ, to have a high correlation with increase in blood pressure.

SUMMARY

. Pronounced shock, fear of stammering and emotions of every kind ·ays brought about increase in brain volume accompanied by in- d size of pulse.

:. Mental work was accompanied by slight congestion in the brain majority of cases with little if any increase in pulse; physical work accompanied by slightly more congestion in the brain with marked rease in size of pulse. Physical work was accompanied by slight odilatation in the finger; mental work, including silent reading, by odilatation in the finger in 38 per cent of the curves, by vasodilata- ι followed by vasoconstriction in 16 per cent, and by no change in per cent.

. Sniffing was accompanied by marked increase in brain volume h increased size of pulse; deep breathing by decrease with slightly reased size of pulse.

. Change of task was accompanied by increase in brain volume with ·eased pulse and was responsible for many temporary rises.

. Normal reading aloud was accompanied by slightly less increase ›rain volume than was silent reading.

. Stammering was accompanied by much more marked increase in in volume than could be accounted for by either the physical or ntal work used in normal speech.

. When the stammerer read as normal speakers do, there was a ırn of the brain volume to normal; it is reasonable to conclude that ·ease in brain volume is an important factor in the production of nmering.

. In order to correct stammering, both the fear of stammering ! the abnormal muscular contractions which usually accompany ımering must be eliminated.

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EXPLANATION OF FIGURES

The top line in all figures is the time line, the second line is t
the third line is the thoracic pneumogram, the fourth line is the h
gram and the bottom line is the brain plethysmogram.

Each notch in the time line represents two seconds unless

A notch on the stimulus line indicates when a stimulus was gi
subject began or stopped reading or speaking, and will be ex
scription of each record.

The top of the pneumogram indicates empty lungs and the bo
just the reverse of the notation in my former monograph (6).

The top of each plethysmogram indicates vasodilatation,
vasoconstriction.

The following curves are all typical reactions; I have avoided
extremes or abnormal curves. If any reader wishes to see my o
is invited to examine those in my album at the Bostom Stammere

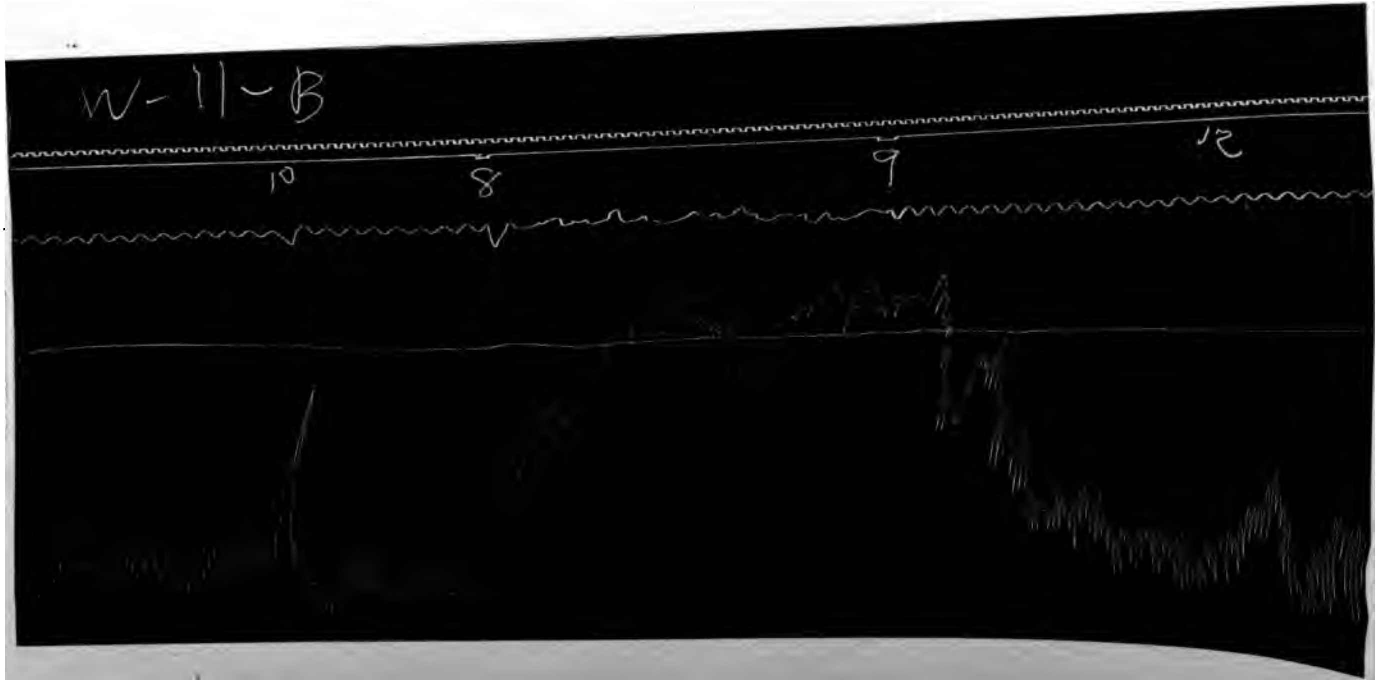

Fig. 4 shows the changes in brain volume during a long period of severe stammering while reading. At *8* the subject began to read aloud and at *9* he stopped reading. At *10* the subject yawned and at *12* I spoke to him. Note the marked rise in brain volume which accompanied stammering; the change in pressure of the writing needle on the drum occasioned by this big rise decreased the excursion of the needle on the drum and thus made a comparison of the pulse impossible. One-half natural size.

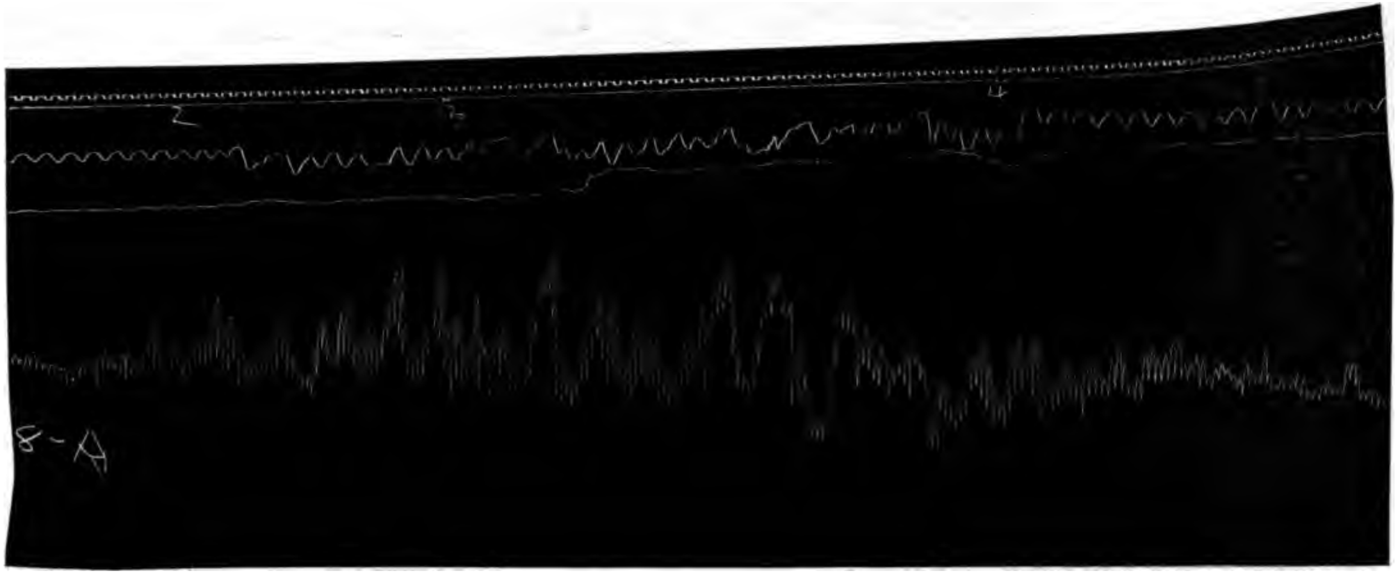

Fig. 5 shows the changes in brain volume and brain pulse accompanying fear of stammering. At *2* the subject was informed that a lady, to whom all stammerers found it very difficult to speak, would enter the room in about fifteen seconds and ask him some questions. She opened the door just before *3* and asked him brief questions from *3* to *4* which he answered briefly, stammering slightly. At *4* he relaxed. Note that both the brain volume and the height of brain pulse increased nearly as much during fear of stammering as during the actual stammering. One-third natural size.

Fig. 10 shows the changes in brain volume accompanying shock. At *W* a shrill whistle was blown unexpectedly. The clock stopped running for a few minutes at the beginning of this curve. Note that the brain volume increased soon after the whistle was blown and that the finger volume remained constant. Two-fifths natural size.

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A NEW OBJECTIVE TEST FOR VERBAL IMAGERY TYPES

BY SAMUEL D. ROBBINS

From the Psychological Laboratory, Harvard University

The purpose of this experiment was to devise an objective test by means of which the vividness of auditory, visual, and kinæsthetic verbal imagery of persons who cannot be depended upon for intelligent introspection may be quickly and accurately determined.

I had for subjects six students who were studying psychology. Four of these subjects were men, two were women. Two other women were subjects only in the first test described. No subject was under twenty years of age or over thirty. All of the women were born and educated in this country and had all their lives spoken English fluently. All the men, on the other hand, were born and educated in foreign lands, had spoken English but a few years, had a distinctly foreign accent, and had limited vocabularies of English words differing much from each other.

A number of series of card pairs containing a single column of five monosyllabic nonsense words of three or four letters were very carefully typewritten (double spaced) so as to look exactly alike except for one word which differed from the corresponding word on the other card in a single letter. The following types of nonsense groups were tabulated separately, the card pairs being shuffled so that the subject could not predict the nature of the next change: (1) the spelling was changed without altering the pronunciation, as veek-veak; (2) two long vowels were exchanged, as zoke-zake; (3) a short vowel was exchanged for the same long vowel, as koss-kose, there being another change, a visual one, in most cases; (4) two short vowels were exchanged, as meb-mib; (5) two consonants were exchanged which gave quite different auditory and kinæsthetic impressions, as aze-ane;

(6) two consonants were exchanged which sounded much alike but gave quite a different kinæsthetic impression in a trial series, as miz-niz; (7) two consonants were exchanged which gave about the same auditory and kinæsthetic impression in a trial series, as bim-pim. As some changes necessitated four-letter words, at least one four-letter word appeared on each pair of cards. There was an equal number of changes at the beginning, middle, and end of the word and for each of the five positions of the word in the column; hence the position of the change could not be predicted.

The nonsense words were given to the subjects in three ways: (1) They were read loud by the experimenter, the subject being careful not to repeat, spell, or visualize the word; (2) the subject repeated each word as it was read to him before the next word was read, being careful not to spell or visualize it; and (3) the cards were exposed in a tachistoscope and the subject was asked to whisper each word. (Subjects were asked to whisper rather than to read aloud because most stammerers can whisper without stammering, and these results are later to be compared with those obtained from stammerers.) The first method permitted the subject to employ auditory imagery alone, the second permitted both auditory and kinæsthetic imagery, and the third auditory, kinæsthetic, and visual imagery. There was an interval of five seconds between the reading of the first card of a pair and the first word of the second card, during which the subject was requested not to think of any of the words on the first card.

The subjects followed this direction remarkably well, and formed surprisingly few associations with these nonsense words. They forgot the nonsense words on the earlier cards before the later cards were read, hence memory of words on the first test played little if any part in the recognition of changes by the second and third methods; changes which were detected when the cards were read by the first method were frequently not detected when the same cards were subsequently read by the same subject by a different method. In the few cases where a change was remembered

from a previous test, the letter 'A' was written on the record so that this change could be omitted in compiling the results if enough associations were formed to alter the averages materially. Each series was read at the speed which each subject found most satisfactory on a trial series.

The subjects were asked to report on a scale of 0 to 3 whether there was a change. 0 indicated that there was no change or that the subject did not know whether there was a change. 1 meant there might have been a change of which the subject was not at all sure. 2 denoted that the subject was pretty sure there was a change. 3 showed that the subject was positive there was a change. If a subject reported the right position of the change, he was credited the score he reported even if he gave the wrong word or could not remember the word. If he reported there was no change when there was a change, his score for the change was 0. As a matter of fact, every card was changed, except when a subject reported so many changes in succession that I feared he would mistrust my instructions that some pairs of cards had one change and others had no change.

The subject was also required to report what the changed word was on both cards, stating how sure he was of the word he reported on the same scale of 0 to 3. For his word score, he was given the score of the word of which he was less sure. If he forgot one of the words or reported one or both words incorrectly his word score was 0. If, on the other hand, he detected the main change correctly but made the same mistake in some minor letter of both words, he was given full credit.

Each subject was given a long trial series[1] by each method before the cards representing the scores compiled in table 1 were used. Subjects were therefore familiar with the experiment before the series reported was used.

Table I. shows the average score each subject obtained in each of the seven groups of changes by each of the three methods of presentation. The four columns in division 6

[1] One trial series consisted of sense words. This series was discarded because the words proved unequally familiar to the men of foreign birth.

Table I

Average Score for Detecting Each Type of Change

Subject	1	2				3				4				5			
	Change Only in Spelling	Long Vowel				Short Vowel for Same Long One				Short Vowel				Consonants Having Different Auditory and Kinæsthetic Impressions			
	W.	L.	R.	W.	Av.	L.	R.	W.	Av.	L.	R.	W.	Av.	L.	R.	W.	Av.
W.	1.50	2.70	2.35	2.95	2.67	3.00	3.00	3.00	3.00	1.75	2.00	2.40	2.05	1.50	2.40	2.40	2.10
E.	1.50	2.70	2.70	3.00	2.80	3.00	2.40	3.00	2.80	1.75	2.10	2.55	2.13	1.40	1.20	1.80	1.47
R.	1.90	3.00	2.95	2.85	2.93	3.00	3.00	3.00	3.00	2.40	1.20	2.95	2.18	3.00	2.20	2.10	2.43
M.	1.70	3.00	2.85	3.00	2.95	3.00	3.00	3.00	3.00	2.30	1.65	1.80	1.92	2.40	1.80	2.50	2.23
D.	1.70	3.00	2.55	3.00	2.85	2.40	3.00	3.00	2.80	1.50	1.50	2.20	1.73	1.00	1.40	2.40	1.60
T.	2.40	2.70	2.70	3.00	2.80	2.00	1.20	2.80	2.00	1.10	1.05	2.75	1.63	1.20	1.50	2.00	1.57
A.	0.60	2.55	1.95	2.40	2.30	2.40	2.40	1.40	2.07	0.80	0.95	0.95	0.90	0.50	0.30	0.90	0.57
S.	1.30	2.15	2.25	1.85	2.08	2.80	2.40	1.80	2.33	1.20	0.80	1.05	1.02	0.20	0.40	1.10	0.57
Av. for men	1.55	2.60	2.46	2.52	2.53	2.55	2.25	2.25	2.35	1.38	1.00	1.92	1.43	1.22	1.11	1.52	1.28
Av. for women	1.60	2.85	2.61	2.99	2.82	2.85	2.85	3.00	2.90	1.82	1.81	2.24	1.96	1.57	1.70	2.27	1.85
Av for all O's.	1.58	2.72	2.54	2.76	2.67	2.70	2.55	2.62	2.63	1.60	1.41	2.08	1.69	1.40	1.40	1.90	1.57

Subject	6				7				8				9			
	Consonants Having Different Kinæsthetic Impressions				Consonants Having Similar Auditory and Kinæsthetic Impressions				Average Change Score for all Types.				Average Word Score for all Types			
	L.	R.	W.	Av.	L.	R.	W.	Av.	L.	R.	W.	Av.	L.	R.	W.	Av.
W.	2.13	2.13	2.40	2.22	1.13	1.60	2.40	1.71	1.98	2.14	2.45	2.20	1.39	1.55	2.22	1.74
E.	1.00	1.73	2.20	1.64	1.40	1.80	1.60	1.60	1.81	2.04	2.27	2.05	1.21	1.88	1.93	1.68
R.	1.07	0.47	2.20	1.24	0.27	0.00	1.20	0.49	2.04	1.49	2.34	1.97	1.76	1.25	2.06	1.71
M.	0.60	1.27	1.53	1.13	1.20	0.53	1.47	1.07	2.02	1.76	2.08	1.96	0.79	0.93	1.47	1.08
D.	0.80	0.80	1.80	1.24	0.67	0.67	1.87	1.07	1.58	1.55	2.26	1.82	0.94	0.85	2.02	1.30
T.	0.60	0.73	2.13	1.16	0.60	0.33	2.20	1.04	1.36	1.32	2.50	1.76	0.58	0.76	2.00	1.15
A.	0.20	0.07	0.80	0.36	0.20	0.60	0.40	0.40	1.06	0.98	1.12	1.06	0.79	0.42	0.73	0.65
S.	0.13	0.07	0.60	0.22	0.13	0.07	0.33	0.18	1.02	0.93	1.10	1.02	0.68	0.68	0.85	0.74
Av. for men	0.50	0.33	1.43	0.86	0.30	0.25	1.03	0.53	1.37	1.18	1.77	1.45	0.95	0.77	1.41	1.04
Av. for women	1.13	1.48	1.98	1.53	1.10	1.15	1.83	1.36	1.85	1.88	2.27	2.01	1.08	1.30	1.91	1.43

give the average word scores for all of the groups taken as a whole; the other columns give the average change scores. In this table there are four columns each in divisions 2, 3, 4, 5, 6, 7, 8 and 9. Of these four columns the one headed L contains each subject's average score in the first test where he *listened* to the words as they were read to him; that marked R contains those in the second test where the subject *repeated* each word after the experimenter; that marked W contains those in the third test where the subject *whispered* the words exposed in the tachistoscope; and that marked Av. contains the *average* of the other three columns. Division 1 is not divided into four columns because the subject only whispered those sets in which the changed word was spelled differently yet pronounced exactly like the first word exposed in the tachistoscope. At the bottom of this table are given the average scores in each test for the men, for the women, and for all eight subjects. Subjects D, E, M and W are women, and subjects A, R, S and T are men.

This table shows that the women as a class did much better in every test than the men and that subjects who did especially well in detecting some change also did especially well in remembering exactly what word appeared on each card, the correlation[1] being + 0.929.

This table shows also that long vowel changes are much more readily detected than short vowel changes; it makes little if any difference whether a long vowel is changed to the same short vowel or to a different long vowel. Conspicuous consonant changes are noted almost as readily as short vowel changes, inconspicuous ones far less frequently. The whispering test gave the highest and the most uniform scores for all subjects. The subject of vowel versus consonant changes will be more thoroughly discussed in a later paper comparing the verbal imagery of normal speakers with that of stammerers.

Columns 1, 2, 3 and 4 of Table II. give the vividness of each subject's verbal imagery on a scale of 0 to 3 as

[1] All correlations were derived from the formula, $r = \frac{\Sigma(xy)}{\sqrt{(\Sigma x^2)(\Sigma y^2)}}$, where x and y are the deviations of the two traits from their mean in any single individual

determined from his ability to detect changes by auditory imagery, kinæsthetic imagery, and visual imagery. These ranks are determined as follows: The visual rank is the average score of the subject in detecting by whispering changes

Table II

Comparative Efficiency of Different Types of Verbal Imagery

	1	2	3	4	5	6	7	8	9
Subject	Auditory	Visual	Kinæsthetic	Total	Spelling Backward	Copying Long Nonsense Words	Copying Perverted and Inverted Print	Writing Perverted and Inverted	Column 8 of Table 1
W	2.5	1.5	1.7	5.7	0.82	5	7	49	2.20
R	2.8	2.0	0.4	5.2	10.12	49	28	3	1.97
T	1.9	2.4	0.9	5.2	1.33	13	77	7	1.76
D	2.3	1.7	1.1	5.1	6.03	1	34	201	1.82
S	2.1	1.3	0.1	3.5	7.25	25	80	20	1.02
A	1.9	0.6	0.1	2.6	22.05	43	157	171	1.06
Av. for women	2.5	1.6	1.5	5.6	3.43	3	21	125	2.01
Av. for men	2.2	1.6	0.4	4.1	10.19	32	106	50	1.45
Av. for all O's	2.3	1.6	0.9	4.9	7.93	23	64	75	1.64
Correlation with col. 4					+0.90 without R	+0.94 without R	+0.90	+0.83 for men	+0.95

in words that are pronounced alike but spelled differently, such as zeat-zeet. The auditory rank is the average score obtained by the subject in detecting vowel changes in all three vowel groups when these are read aloud to him. Trial experiments showed that kinæsthetic imagery played little if any part in these groups and that it actually reduced the score in many cases. The kinæsthetic rank is the average score the subject obtained when he repeated after the experimenter those cards in which changes had been made in words which sound much alike but give quite a different kinæsthetic impression (such as vad-vab), and in which he failed to detect changes when they were read aloud to him. The kinæsthetic and auditory imagery are so closely connected that I could find no better way to separate them. Trial series showed that there is little likelihood the subject will be able to detect a second time a slight auditory change which he does not detect the first time by the same method. The

changes in this group were so chosen from the introspections on a trial series as to reduce to a minimum the auditory element, and to make the kinæsthetic element as prominent as possible. There is a close correlation, + 0.950, between the average ranks of subjects for the entire series as recorded in the average column in division 8 in Table I. and the total ranks obtained by adding together their three ranks in columns 1, 2, and 3 of Table II. as recorded in column 4. It seems to make little difference, therefore, what type of imagery is employed for the test as a whole; the subject possessing the greater sum total of verbal imagery attains the higher rank in this experiment.

Before beginning any tests, each subject was asked to answer the questionnaire given on pages 195–200 of E. B. Titchener's 'Experimental Psychology, Student's Manual, Qualitative.' A careful examination of the question blanks showed there was no correlation between verbal and non-verbal imagery. It is incorrect to assume, therefore, that because one has very vivid non-verbal imagery of a given type he must also have very vivid verbal imagery of the same type.

The four experiments which follow were performed to confirm the reliability of the preceding test.

After the test with nonsense words had been completed, each subject was asked to spell backward twenty words of increasing length, beginning with two letters and ending with twenty-two, and to report the method he employed for both the short and the long words. The experimenter kept a record of the time spent in spelling each word and of the number of errors. It is obvious that the efficiency of a subject in performing such a task depends upon both the speed and the accuracy with which it is accomplished; the greater the speed and the fewer the mistakes, the higher the efficiency. If this efficiency is represented by the product of the time in which a unit of the task is performed by the number of errors made, the lower product will denote the greater efficiency. These products for the six subjects who performed this test will be found in column 5 of Table II.

It will be seen that, with the exception of R, the lower product corresponds in every case with the higher sum total of verbal imagery recorded in column 4, the correlation without R being + 0.90. This shows that efficiency in this test is proportional to the total amount possessed by the subject of verbal imagery of types useful in performing the task. R reported that he employed visual imagery alone for this test because he believed it would increase his speed. As he possessed the strongest auditory imagery of the six subjects and as the trial series showed him to have about the weakest visual imagery, he should have employed auditory imagery or a combination of both, rather than visual imagery.

After spelling backward, each subject was asked to copy fifteen long nonsense words of from twelve to twenty-eight letters, to make a dash every time he looked at the copy, and to report how he copied these words. The experimenter kept a record of the time spent in copying each word and of the number of errors. Column 6 of Table II. gives the relative efficiency of the subjects in this test, this being the product of the average time in seconds it took the subject to copy each word by the average number of mistakes per word; and, as in the spelling backward test, the lower the product, the greater the efficiency. With the exception of R, the lower product corresponds closely with the higher sum total of verbal imagery, the correlation being + 0.94 without R. R employed auditory imagery alone, looking at the first twelve words but once and spelling them as he pronounced them. As long groups of consonants made it impossible to pronounce these words, he naturally made many mistakes.

After copying these long words, each subject was asked to copy two or three lines of each of six type-written selections, arranged in the following ways: perverted, backward perverted, inverted, backward inverted, perverted and inverted, and backward perverted and inverted. By backward I mean spelled from right to left instead of from left to right. By perverted I mean written so as to be read in a mirror. By inverted I mean written so as to be read by one facing the writer. The subjects introspected as in the previous

tests, and the experimenter kept a record of the time it took to copy each line and of the number of mistakes. Column 7 of Table II. gives the relative efficiency of subjects in this test. This efficiency is denoted by the product of the average time per line in minutes by the total number of mistakes; and here again the less the product, the greater the efficiency. With the exception of T, the lower product corresponds in every case with the higher sum total of verbal imagery, the correlation being + 0.90 even with T included. T used an unfortunate method on the first line which caused him to make nearly as many mistakes as he made on the other twelve lines combined; when this one line is discarded, his score becomes practically equal to D's.

After copying these unusual kinds of printing, the subject was asked to write 'United States of America' so that it would be read normally by a person facing him; 'Harvard University' so that it would be read normally *through* the paper without inverting it; 'Cambridge, Massachusetts' so that it would be read normally through the paper *inverted;* and 'European War' so that it would be read forward *through* the paper without inverting it but with each letter perverted. The subject introspected as before and the experimenter kept a record of the time required to write each phrase and of the number of errors. Column 8 of Table II. gives the relative efficiency of the subjects in this test. Here the product of the average time per letter in seconds by the total number of mistakes gives the efficiency; so, as usual, the smaller product denotes the greater efficiency. With the exception of the women, who found this test far more difficult than did the men, there is a fairly good correlation, + 0.83, between the efficiency of each subject in this test and his total verbal imagery score.

As all of the spelling, copying, and writing tests just described require the coöperation of auditory, kinæsthetic, and visual verbal imagery, the high correlation between the efficiency of a subject in these tests and his total verbal imagery score (which is the sum of the scores of the separate types of verbal imagery used by a majority of the subjects in

performing each of these tasks), shows that the efficiency of subjects in performing a given task is proportional, approximately, to the sum of the scores of the separate types of verbal imagery commonly employed in performing that task.

Any one wishing to use a similar but shorter test can score auditory imagery by reading aloud to his subjects thirty cards such as I used containing the following vowel changes: koss-kose, nume-nurm, dack-dake, girn-gine, neff-neaf, afe-ofe, eag-oag, ane-une, ipe-epe, fese-fose, bose-buse, nafe-nufe, vife-vefe, obe-ube, zoke-zake; ank-enk, ald-uld, nef-naf, ilt-ult, nop-nep, baf-bof, eft-uft, ilm-ulm, zep-zop, alk-olk, maz-muz, oln-uln, mev-muv, tig-teg, zab-zib. These cards should be well shuffled with one another and with a second group containing at least twenty consonant changes which give a distinctly different kinæsthetic impression such as the following: ubs-uds, vab-vad, nis-niz, gem-gen, bis-tis, vot-vob, sef-zef, tov-pov, ips-ids, zup-zut, pax-dax, mub-nub, abf-atf, ost-ozt, emk-enk. These groups should then be repeated after the experimenter, and in the case of the kinæsthetic score the record should be kept only of those consonant changes detected that were not noticed when the experimenter first read the second group. The third group, used for scoring visual imagery, should contain at least ten nonsense words which are spelled differently yet pronounced alike such as mije-mige, bick-bik, fis-fiss, gax-gaks, veet-veat, doxe-doax, klab-clab, zoll-zol, boze-bose, sibe-cibe. After the subject has thus repeated the first and second groups after the experimenter, the third group should be shuffled with ten pairs from other groups and exposed in a tachistoscope long enough for the subject to read each card aloud. The experiment should be conducted and the results scored as outlined at the beginning of this report.

Summary

1. Women possess more vivid verbal imagery than men.
2. Long vowels receive more attention than any other letters.
3. Short vowels receive much less attention than do long vowels.

4. Consonants receive less attention than vowels.
5. A change of one letter in a pair of nonsense words can e detected most readily if the subject reads the words aloud ·om copy.
6. Those persons will perform a given mental task most ficiently who possess the most vivid types of verbal imagery ›mmonly employed to accomplish that task.

NOTE ON THE VERBAL IMAGERY OF STAMMERERS AND NORMAL SPEAKERS.[1]

I stated that the subject of vowel versus consonant changes ould be discussed in a later paper comparing the verbal nagery of normal speakers with that of stammerers. As my kperiment shows that the average verbal imagery for twelve ormal speakers and twelve stammerers is practically idencal, a brief note will suffice to summarize my results.

The same objective test described in the above named rticle was given to four additional speakers, two of them men, nd two women, and to twelve stammerers, six of them men, nd six women; and the average score for the twelve normal ıbjects and the twelve stammering subjects was compiled as ı my earlier article.

The average change score showed that the stammerers deected changes a very little better than the normal speakers hen the card pairs were read to them, 1.7 compared with 1.6, nd that the normal speakers detected changes a little more eadily when they looked at the card pairs and whispered hem, 2.0 compared with 1.8. They averaged the same, 1.5, hen they repeated the card pairs after the experimenter, and veraged the same, 1.7, when all three methods of presentaon were averaged together.

The vividness of each subject's visual, kinaesthetic, and uditory verbal imagery was determined as in the earlier paper nd the stammerers and normal speakers were found to have he same average auditory imagery, 2.2, and the same average inaesthetic imagery, 0.8. The visual verbal imagery of the

[1] Prepared after the article was in type.

stammerers was but 75 per cent. of that of the normal speakers, however, 1.2 compared with 1.6. As few persons employ visual verbal imagery to any extent in speech, there is obviously nothing in verbal imagery of any kind to account for stammering. There is a suggestion, however, that certain letters may attract the stammerer's attention unduly; this problem I am now investigating. The following facts came out in this investigation.

The stammerers did not detect the long vowel changes so readily as the normal speakers, 2.4 compared with 2.7 in the listening test and 2.2 compared with 2.6 in the repeating test; yet they detected the short vowel changes more readily, 1.9 compared with 1.6 in the listening test and 1.7 compared with 1.4 in the repeating test.

Averaging the three series composed of the three types of vowel changes I found that the stammerers and normal speakers scored alike, 2.2, on the listening tests, and that the stammerers did not do quite so well as the normal speakers on the repeating tests, 1.9 compared with 2.1, or on the whispering tests, 2.4 compared with 2.5.

Averaging the three series composed of the three types of consonant changes I found that the stammerers excelled in the listening tests, 1.15 compared with 0.95, and on the repeating tests, 1.05 compared with 0.95, but scored only 1.44 compared with the normal speakers' score of 1.68 on the whispering tests, the difference being due, no doubt, to the better visual imagery of the normal speakers. The average for the three methods of presentation was the same for normal speakers and stammerers, 1.19. The maximum and the minimum scores averaged the same for stammerers and normal speakers in the vowel changes and the maximums averaged the same in the consonant changes, but the normal speakers' minimums averaged twice as low as the stammerers in the consonant changes. It would seem, therefore, that stammerers pay more attention to consonants than do normal speakers; this is confirmed by the work I have done in connection with the correction of stammering.

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Psychological Monographs

The Interference of Will-Impulses

With Applications to Pedagogy, Ethics and Practical Efficiency

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NOTE.

This monograph constitutes the first part only of the investigation on the interference of will-impulses. The second part dealing with the complex processes of writing lapses is to appear separately.

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To the late Prof. Münsterberg, I am greatly indebted for guidance in some of the difficult steps necessarily involved in the course of this investigation. Thanks are also due to Prof. Langfeld who not only acted as a subject in the experiment for two years, but whose suggestions have been extremely helpful in the working up of the results. Prof. Holt by his criticisms after a careful perusal of my work, at the time it was submitted as a dissertation, has laid me under further obligation, while it is scarcely necessary to mention my debt to the subjects who willingly participated in the "most difficult problem" in the laboratory. I am also grateful to Prof. Warren who has placed his library at my disposal and has been most obliging in other ways. Acknowledgment must also be made of the fact that Miss Rose J. Ginsburg assisted in the tabulation of the results, while Miss Virginia H. Bartlett prepared the index of names and subjects.

INSCRIBED TO

PROF. J. W. A. HICKSON

WHO FIRST TAUGHT ME THE ELEMENTS OF

SCIENTIFIC METHOD AT

McGILL UNIVERSITY

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MUTUAL INTERFERENCE OF WILL IMPULSES

PART I

CHAPTER I

INTRODUCTION

It is only within the last decade or so that the psychology of movement and the broader aspects that it involves has been coming to its own. Thus in 1899 we find Woodworth saying:

"In all sorts of psychology, save one, there is of late an increasing interest in the motor side of consciousness . . . . In view of all this interest, it is somewhat surprising that . . , we have as yet no psychophysics of the voluntary movements,"[1] while H. L. Hollingworth, two years later, opens his monograph,[2] which is on the complementary phase of Woodworth's problem, with the following paragraph:

"The student of the psychology of movement is, to say the least, not hampered by the novelty of his subject. Ever since the days of the muscle sense controversy, investigator after investigator has interested himself in the subject of movement until a considerable body of motor psychology . . . has developed."

Since those words were written, the motor phase of psychology has kept steadily advancing, receiving its impetus from various quarters and forming the basis for new tendencies in that science. Our text books no longer discuss the subject of "action" as if it were a mere appendix to "will" or "habit"; and just as perception and its various adjuncts would formerly form the basis for many an all-embracing theory, so now "history repeats itself" in the domain of movement. Indeed, there is even a tendency to barter consciousness for movement among a certain type of psychologists.

[1] B. S. Woodworth: Accuracy of Voluntary Movement, p. 1. *Psychological Review,* Monograph Sup., vol. III.

[2] H. L. Hollingworth: Inaccuracy of Movement. Archives of Psychology, vol. II.

And yet with all the due and, in some cases, undue emphasis on the motor side of psychology, there is no denying that it has always to catch up with the ground traversed in the sensory sphere; and this in spite of, or perhaps rather just because of the fact that ontogenetically movement is prior to consciousness. The underlying motive of this neglect seems to be the argument that since psychology is the science of consciousness, the consideration of movement has no place here—thus overlooking the fact that "the transition from reflex action to volitional is not abrupt and short."[3] With slight modification, the statement made by Woodworth in his dissertation nearly 20 years ago, that "we have nothing in this line that can compare with the immense amount of work done on the relation of perception to the stimulus perceived, or . . . that can compare in completeness with the work done and still doing in all departments of sensation"—this statement holds good even to-day.[4] Particularly is this true to-day of the psychophysiological phenomena included under the head of inhibition and springing from a volitional source.

So far, inhibition has primarily been a subject of discussion for the physiologist, while the so-called "inhibition of ideas" has made it appear as a relic of a by-gone age in the development of psychology.[5] Ever since Sherrington published his first results on the rôle of antagonistic muscles in movement, numerous investigators have followed along the same track, and it is to them that psychologists look up. The results of such investigations need not be underestimated, but they are within the scope of psychology only in an extended sense. It is important that the psychologists take cognizance of the facts disclosed by such investigations, but outside of this, they do not form the subject-matter of psychology. Psychology, as we shall see presently,

[3] Sherrington: Integrative Action of the Nervous System, p. 389.

[4] It is interesting to note that of all the psychological primers and text books known to the writer, only one syllabus, by Heath Bawden, reverses the conventional order of topics, and begins with the outline of action instead of reserving it to the end.

[5] *Cf.* B. B. Breese: On Inhibition (pp. 6-17, *Psychological Review*, Monograph Supplements, vol. III) for a historical summary of the views on psychological inhibition and a brief but concise discussion of its status as a term in contemporary psychology.

has its own problems in inhibition; and with the exception of sensory inhibition, these problems have received little attention, if any, at the hands of the experimentalist. Sensory inhibition, too, has by no means received anything like adequate treatment in the laboratory.

Partly the deficiency is due to the *dynamic* character inherent in all phenomena of inhibition. Theoretically we have rid ourselves of the implications of the old faculty psychology; *practically,* however, the average psychological investigator is still drawn along by the momentum of an old impulse. Broadly speaking, it may be said that the study of sensation, perceptions or feelings is, in the minds of most of us, connected with the study of facts (elements). To study inhibition is to study acts (relations). That is one reason why the cognizance of inhibition as a psychological phenomenon was so late in developing. Inhibition involves a community of sensations, memories, impulses, etc. The very circumstance that brought about the growing interest in association theories contributed to keep the facts of inhibition in obscurity. Association was recognized as a "force" as early as the period of Hume, because it gave evidence of governing the "faculty" of memory. It would have been difficult to conceive of such a negative thing as inhibition in connection with some faculty or ability. Association was the attraction of one idea to another. There seemed to be no room for inhibition, because there was apparently no use for it. It was left for physiology to lead the way in emphasizing the importance of the inhibitory mechanism, until gradually a new stratum began to form in psychology, and sooner or later inhibition is bound to be reckoned with in the ordinary text book as a fundamental relational category bearing on all the elements of consciousness.

There is a further drawback in the study of *impulse* inhibitions. The field of volition is not a clear-cut segment of psychology. With the exception of thought, there is more misgiving here than in any other part of general psychology. The crux of volition is shifted from one thing to another in the various text books and treatises. Thus it is that the problems subsumed under the possible head of volition are linked on the

one hand with the most abstruse philosophical speculation, while on the other hand, they are transferred to the physiological laboratory[6] as if it were all a matter of muscle and nerve alone.

The play between these extremities is so great and is beset with so many difficulties that from one of the main results in this very investigation,* the neglect of this field of psychology might be expected. In turning to sensation, perception, imagination, and even affection, there is a feeling that the ground is at least a *terra cognita*. Volition does not present that aspect. And yet the concept of will is second to none in importance from the standpoint of a cultural philosophy. Those very psychologists who subordinate this concept to what they consider a more palpable, and therefore, scientific term, do not fail to make much ado about this time-honored notion of *will* as soon as they need it for other purposes, such as emphasizing its value as a principle in education.

Will, together with all the phenomena generally included under its head, is undoubtedly a factor in life that cannot be overestimated. That, however, is true from an intentional point of view. The negative counterpart of will, viz., inhibition, is the factual and explanatory portion of the story. Volition is possible only because inhibition is possible. The former posits the the latter. Not only is this true of the formal relation between these two concepts, but also psychologically.

In the most general terms, an impulse is an idea that obtained its promotion at the expense of its congeners, *i. e.*, it could only become an impulse after, and because all the other elements in consciousness have been inhibited.[7] If this inhibition lasts sufficiently long, the impulse realizes itself into action. We say then the agent has willed the act. Had other ideas inhibited the

[6] This brings to mind that as early as 1865 the temporal relation of voluntary acts was studied in the Tübingen Physiological Laboratory by W. Camerer, who published his dissertation "Zeitlicher Verlauf der Willenshandlungen" in 1866.

* *Vide infra*, p. 37.

[7] The "double reciprocal innervation" of antagonistic muscle may be regarded as the purely physiological manifestation of the same principle. *Cf.* Sherrington: Integrative Action of the Nervous System, p. 83 ff.

usurping idea, the impulse would not have been willed, hence not realized.

In spite of the great importance of this correlative of will, it is only natural that the positive concept should have remained the magic word to this very day. Our practical interests always take precedence over our scientific demands, since analysis as an activity is only a part of and not co-extensive with life. In life we are constantly on the lookout for results and scarcely ever for the indispensable conditions that bring about these results. Hence the Will that is so prominent in the history of philosophy, and especially in ethics, the very concept which forms the basis for all non-scientific literature including history, rhetoric and biography, will ever remain supreme in its proper sphere. For the man who is concerned with interpretation or exhortation cannot but take recourse to such a concept; and that is what we are doing all the time we are not engaged in describing or explaining phenomena.

It is perhaps owing to this universality of the popular concept "will" that psychologists generally have adopted the word "volition" to differentiate the psychological sense from the broader use of the term, and for the unit of volition let us take the will-impulse or simply the impulse in its generic sense.

If the correlative principle in volition were to be stated in the briefest form, it might be as follows: "no will impulse without inhibition" (of other possible or incipient impulses). Let us go a step farther now, and ask ourselves: what happens when there are two or more impulses, perhaps a whole series of them, going on almost simultaneously or in quick alternation? It will be evident that we have here a more complex and more interesting situation than in the first case; this leads us to the formulation of the problem of our investigation.

Note on Impulse.

It is of course evident that the word "impulse" is not used here in the popular sense as corresponding to the German word "Trieb." In many text books impulse is identified with striving. Thus Höffding (Outlines of Psychology, p. 322) says "In impulse

proper there lies always a more or less conscious demand." In our usage the term impulse may be defined as "any tendency to action." Unquestionably it would be desirable to have two distinct terms for the two different usages, but there is no reason to suppose, in the present stage of psychology, that what conditions the impulse psychologically is: "that with the momentary feeling and sensation, there should be combined a more or less clear idea of something which may augment the pleasure or diminish the pain of the moment." (Hôffding: loc. cit.) The psychological state that Hôffding is describing is of biological and ethical significance rather than psychological; and it must be apparent that if we have one word to designate two psychological states, the simpler and more elemental one should get the preference.

Referring to the impulsive act as it is commonly called, Wundt says: "Willenshandlungen von dieser Beschaffenhait pflegt man *Triebhandlungen* zu nennen, und die ältere Psychologie hat sie meist als spezifisch verschiedene Vorgänge dem Willen gegenübergestellt.

'Die einfache selbstbeobachtung lehrt jedoch, dass in allen den Fällen, wo es sich um einen wirklichen Bewusstseinsvorgang, nicht etwa um eine blosse Reflexbewegung handelt, zu einer solchen Scheidung nicht der allergeringste Grund verliegt."[9]

A determination of the terms we are handling is really necessary at the very outset in order to avoid possible misunderstading subsequently.

Personally the writer is in favor of applying the term impulse in four different senses to cover the demands, however, of four different sciences or disciplines. Thus we may talk of a physiological impulse, a psychological impulse, a biological impulse, and an ethical impulse. In the physiological sense it would be the mere nerve impulse. All reflex action would come under that head only. The psychological impulse should include all tendencies to action which are accompanied by consciousness and should form the unit of volition, whether that tendency be more like an impulsion and thus fought against by the individual, or whether it follows a process of careful deliberation. The impulsion may not be identified with the will of the individual, but it is a volition none the less. There remain the biological impulse, which is often used to designate a certain phase of instinct, as when used in connection with sex, Höffding's definition fitting in here quite well, and lastly the ethical impulse, which empha-

sizes not so much the act as the object or rather the whole situation in question.

There is no harm in employing a term to connote different things in accordance with the requirements of different departments of knowledge, provided that the different connotations are distinguished and agreed upon.

[8] A similar view is found in Külpes' Outlines of Psychology, p. 322.

[9] Wundt: Grundzüge d. Phys. Psychologie, vol. III, 5th ed., p. 247.

CHAPTER II

THE PROBLEM STATED.[10]

Although the title of the investigation might easily convey an idea of the nature of our task, it might be of some advantage to state the problem in some such form as this: Given two kinds of impulses in rapid alternation in what way do they affect one another?

Such a situation is certainly not rare. It is not only in a case of emergency where one is apt to "lose his head" because he attempts to do several things at a time, but even in our daily routine we often find ourselves "crowded." More work is planned than can be done under normal conditions. At times we are like the British premier who would rise half an hour too late every morning and then rush about in the attempt to make up for the delay. Again we may be in the position of one who has a definite and peremptory task before him which is physically almost impossible to perform in the time set for it.

It will be seen at once that the principle here involved might have far-reaching ramifications, bearing in its practical applications on executive ability, pedagogy, and possibly the study of abulia and allied mental abnormities. In industrial efficiency the question looms even larger. Many failures, accidents, and indeed, catastrophes, are due to the fact that a task calling for quick action on the spur of the moment overtaxes the ability or skill of the responsible person. We have only to bring to mind the every-day experience of the sea captain or the motorman to realize how momentous it is to grapple with a problem that is at the basis of our very safety.

In the opinion of a director of one of the largest steamship companies, two types of ship officers are largely to blame for

[10] The investigation was begun at the suggestion of Prof. Münsterberg, by Dr. V. V. Anderson, Boston Municipal Court Physician and Psychologist who, however, did not complete the experiments, and has not published any of his results.

accidents at sea. "They all know exactly what is to be done in every situation" this keen observer told the late Prof. Münsterberg whom he was consulting about ship-service discrimination tests, "but there are too many who do not react in the appropriate way when an unexpected combination of factors suddenly confronts them, such as the quick approach of a ship in the fog . . There are ship officers who know the requirements excellently, but who are almost paralyzed when the dangerous conditions suddenly threaten. Their ability for action is inhibited. In one moment they want to act under the stimulus of one impression, but before the impulse is realized, some other, perhaps rather indifferent impression, forces itself on their minds and suggests the counteraction, and in this way they vacillate and remain inactive until it is too late to give the right order or to press the right button. The other type feels only the necessity for rapid action, and under the pressure of greatest haste, without clear thought, they jump to the first decision which rushes to their minds. Without carefully considering the conditions really given, they explode in an action which they would never have chosen in a state of quiet deliberation. They react on any accidental circumstance, just as at a fire men sometimes carry out and save the most useless parts of their belongings."[11]

Considering the similarity of the general situation, we should suppose that the same observations might apply *mutatis mutandis* to the motorman, the railway engineer, the army officer, and, in brief, all who are intrusted with some delicate task involving many factors of an unpredictable nature. The statesman, the public speaker, the business man, the attorney, are constantly encountering difficulties in the form of rapidly changing situations. A quick repartee may carry the day for one. An ineffective move may spell disaster.

Thus the practical side of the problem is important, indeed, but our primary interest is theoretical. We shall consider first: *What are the phenomena, both conscious and unconscious, resulting from a condition of rapidly alternating impulses?* The practical considerations that may be deduced from the results will be taken up towards the end.

It must further be understood that we are not concerned here with the problem of moral choice between two duties or tasks.

[11] Münsterberg: Psychology and Industrial Efficiency, p. 84.

Undoubtedly the painful scruples and vacillation of Hamlet, the inner struggle of Antigone, the sufferings of Werther have some connection with the issue at hand. In comparing the details of the two sets of problems, we may find a certain overlapping with regard to the determining processes, but there is at least this great difference between them. In the one, the result hinges on *time* as the determining factor, the aim in the first instance being to complete the *whole task;* while in the other the condition *ex-hypothesi* involves a *moral choice* between two things, though both of them may be *desired* by the agent.

The conflict of impulses that we experience in every-day life when we have before us two courses of action brings us closer to the issue at hand. Some of these conflicts, because of the consequences that are at stake, may be grouped under the class just mentioned. Even in those cases where the decision is finally made on a purely prudential basis, psychologically the principle is the same as in the most striking ethical situation.

A great many of the decisions we have to make are, however, not related to moral matters, and those individuals who are not slaves to habit and have no decided preference will constantly find themselves confronted with the task of making a decision which, for the expenditure of energy it often calls for, is not at all commensurate with the importance of the consequences either way. "Shall I walk to the waterfall to-day? Or shall I ramble along the seashore?" is the illustration Herbert Spencer[12] gives us of a morally indifferent end. For the moral agent it may be entirely indifferent. Not so however, for the psychological re-agent in whose mind a veritable conflict may occur.

Probably this is the type of behavior referred to in the following description of a conflict. "Adults sometimes experience such a state when two opposing courses are open to them and they have no decided reason for following either; they feel impelled first to one, then to the other, and in pathological cases the forces may be so evenly balanced that action is prevented altogether. As a rule, however, the permanent motive ideas are so numerous and so far-reaching that every suggested course of action is either helped or hindered by them."[13]

[12] Data of Ethics, p. 6-7.

[13] Mellone and Drummond: Elements of Psychology, p. 173.

There is much in common between such a conflict and the issue at hand, but there is at least this difference in their respective conditions. In the common every-day situation, the agent knows that only one of the impulses can and need be carried out, while in the experiments to be described, the subject makes the ***endeavor to carry out both sets*** of impulses, and if a choice *is* made from the sheer physical impossibility of accomplishing the whole task, it is done in spite of his intentions. It is just this difference that distinguishes our investigation from those on choice such as Ach's and Michotte's researches (to mention only a couple of the more recent studies among the large number of investigations initiated by Donders).

Special emphasis must be laid on such a distinction in view of the circumstances that the field we are dealing with has been treated as a common trespassing-ground by speculative psychology, ethics and allied disciplines. Whether we should be disposed to go the length of Stout who claims that voluntary action in the sense of deliberate action (selective action probably according to Titchener) involves conception of the Self is quite a different matter, but at any rate, it is not difficult to concede that the status of deliberate action and non-deliberate action may possibly be determined by different variables. And when Stout draws the sharp dividing-line between "voluntary action" and "impulsive action" and also between "deliberation" and "conflict of impulsive tendencies" by saying "When two disconnected impulses simultaneously prompt to incompatible courses of action, if the conception of Self does not come into play, one interferes with the other in a quasi-mechanical way"[14] it must immediately become evident that the above-described type of action is what we are studying. It is the very "brute strength between them' as Stout puts it, that we are trying to compare; and from an experimental point of view it is something to be thankful for that it is capable of working out not only in a *quasi*-mechanical but in a thoroughly mechanical way.

Another preliminary point must be cleared up here. In the introduction of this study the impression may have been gath

[14] Stout: Manual of Psychology, 3rd ed., p 06

ered that the writer had set out in the first place to investigate the nature of inhibition of voluntary impulses. Strictly speaking, this would have been an assumption, for the term inhibition has a more or less fixed usage in psychophysiology. As a matter of fact nothing was taken for granted. There was no theory to be given experimental confirmation, no foregone hypothesis to be tested out. The work was undertaken with no theoretical bias, a circumstance which has its disadvantage as well, though, in the long run, it is not as great as its advantage.

From education and observation we of course know that "no one can serve two masters well." Julius Caesar, it is said, was able to dictate several letters at one and the same time, but, granted that this fact about him was really not exaggerated, the persons who would have a claim to be classed with Caesar are, to say the least, very few.

Probably, if we were asked off-hand whether we thought two or more different acts or impulses would interfere with one another if aroused almost at the same time, we should be inclined to reply in the affirmative. It is very rare to find this sort of interference referred to in text books of psychology. LADD and WOODWORTH do mention it, but that is about as far as they get in saying:
"When a series of dissimilar acts must be performed in quick succession, one of these is likely to interfere with another."[15]

We may even be certain that the acts interfere with one another, but this does not mean that they necessarily inhibit one another; for interference is a broader concept than inhibtion. Interference is used extensively in physics in connection with the study of light, sound and electricity.[16] It also figures in philology[17] and in (pure) physiology, [18] as well as in sociology[19] and psychology.

[15] Ladd and Woodworth: Physiological Psychology, p. 540.

[16] The phenomena of interference in physics are demonstrated in every elementary course and discussd in every text book.

[17] Hanns Oertel: Lectures on the Study of Language, p. 205: "The real causes which bring about those phonetic changes....are change of speed, which interferes with the proper co-ordination of movement."

[18] Especially in Sherrington's work.

[19] C. Tarde in his "Les Lois de l'Imitation" (p. 32), talks of "*interférences-*

Two beams of light interfere without producing necessarily negative result. Under certain conditions colored bands ap-ear. In the interference of sound this feature of the case is ven more striking. Two sets of waves may interfere and yield tone of increased intensity instead of producing silence which uld be the analogue to inhibition in the psychophysiological p e.

In the development of language "Increase of speed leads by o means always to simplification or loss of sound. The opposite occurs."[20]

As regards the interference between a sensory stimulus and motor impulse, LEHMANN tells us that "Sind die beiden Ersch-inungen gleichzeitig, hemmen sie sich; geht aber die eine der nderen voraus, *bahnt* die erstere die letztere an,"[21] citing the ollowing instance: if a subject manipulating an ergograph in a egular tempo should unexpectedly hear a pistol shot shortly efore the muscular contraction had begun for the lifting move-ent, the next two or three lifting movements would as a rule e considerably greater.[22]

SHERRINGTON is inclined to talk of interference as close akin inhibition, but even his language, as may be evidenced from ch statements as "There is interference betweeen the two reflex-

mbinaisons" and "*interférences-luttes,*" thus showing that he makes a dis-nction between conflicting and non-conflicting interferences.

[20] Hanns Oertel: Lectures on the Study of Language, *loc. cit.*"

[21] A. Lehmann: Grundzüge der Psychophysiologie, p. 436. *Cf.* also Vundt Grundzüge der physiologischen Psychologie, vol. I. p. 86 (5th lition) on interference of stimulations and I. p. 243. Interference psychology is most frequently spoken of in connection with asso-ation, but more recently it has been studied in connection with memory nages of MEAKIN (Mutual Inhibition of Memory Images, Harvard Psy-iol. Studies I), in connection with habit by McMEIN and WASHBURN Effect of Mental Type on the Interference of Motor Habits, *American urnal of Psychology*, 1909, 20) and by WARNER BROWN (Habit Interfer-ice in Card Sorting, University of California Public. in Psych. 1914, I), by liss KLEINKNECHT in relation to Optical Stimuli (Interference of Optical timuli, Harvard Psych. Studies, vol. II) and by KELLOG, in connection with e feelings (Alternation of Feelings, Harvard Psych. Studies IV).

[22] *Cf.* especially L. Hofbrauer: Interferenz zwischen Verschiedenen Im lsen im Centralnervensystem. Arch[illegible] fü die Gesamte Phys. (Pflüger) l. 68, 1897, pp. 585 and 586

es, and the one is inhibited by the other"[23] and ". . . There is an interference which is tantamount to, if not the same thing, as inhibition,"[24] implies that interference is the broader term, and is at least a stage prior to inhibition.

In the opinion of the writer, interference has reference to the *condition,* while inhibition is applicable to the result. It is thus legitimate for us to assume at the outset that the phenomena that we are looking for in our investigation would be those of interference, but having satisfied ourselves as to that purpose, there remains nothing but to grope about for preliminary data.

The Approach.

In a problem such as the one before us, two possible modes of approach are possible. The one is quantitative, the other qualitative. In the former we are concerned mainly with the accumulation of data. There is no wide scope here for close analysis of the phenomena. The variations of the experiments all center around the idea of exhausting one line of procedure, the experimental situation being reduced to the lowest terms. The chief advantage of the quantitative approach is the decisiveness of the results whether positive or negative. In going over traversed territory, in testing out a theory or in dealing with a problem that requires for its solution the answer "yes" or "no", the quantitative method is the more serviceable one and is actually used just for those purposes enumerated.

The qualitative method which demands a more complex experimental situation does not yield so clean-cut and decisive results as the quantitative method, and more often calls for the aid of interpretation. It has this advantage, however: it affords an excellent opportunity for orientation in a problem, and the results it leads up to may point to new vistas that might have been left undetected in the other procedure.

In our study of interference both methods were employed. Simple movements constituted the task of the subjects for a time. The data spoke in hundreds of thousands. After a year

[23] Sherrington: Integrative Action of the Nervous System, p. 135.
[24] *loc. cit.*, p. 138.

of experimentation along this line, it became clear, however, that the vast array of figures wanted supplementing, and that the second mode of approach might shed some important light on the quantitative results.

That was just what turned out to be the case. It will be seen later in what way these two methods acted as complementary to each other, but for the present, it is only necessary to point out that, owing to this circumstance, this study has been divided into two independent parts. This does not mean, however, that we are dealing with two different parts of a problem. Distinct as the two methods are, they only brought out two different phases of the same problem. It would be quite permissible to treat the results yielded by both methods under the same rubric; but since the stimuli used and records obtained were dissimilar, it was thought best to treat all the subjects separately in the two parts, and thus avoid confusion. The data in the first part of the study will be referred to as the simple movement experiments; those in the second part will be called the graphic experiments.

CHAPTER III

Survey of Literature

The monographs and articles that might be said to form tangential contacts with the problem in question are numerous, but very few go beyond that point, and to the knowledge of the writer, there is not a single one among them that touches the central issue directly. No wonder TITCHENER says, referring to what is known about the conflict of impulses, "Here, however, psychology is sorely in need of further analyses."[25]

In the following paragraphs only the investigations that have some bearing on the first part of our study will be mentioned. Those dealing with the more complex movements will be reviewed in connection with the qualitative phase of our work.

Probably the first investigator who was on the way to study the influence of one motor impulse upon another was M. L. PATRIZI,[26] who set himself the task of ascertaining whether the "brain is more easily fatigued in sending out to the two halves of the body a series of double simultaneous impulses than in issuing an equal sum of unilateral impulses towards the right and left (periphery)."

With PATRIZI, the problem is more of a physiological character, following up the work of Mosso. He had his subjects use two ergographs, one for the middle finger of the right hand, and one for that of the left hand. At the two-second beat of the metronome they were to flex the two middle fingers. The main result here was that the succession of two voluntary impulses carried out by symmetrical members is more favorable for the central nervous system than for these impulses to be carried out simultaneously.

His conclusion is: "Il résulte de la présente étude que, le fait

[25] Titchener: Text-book of Psychology, p. 460.

[26] La Simultanéité et la Succession des Impulsions volontaires symétriques. Arch Italiennes de Biologie vol. XIX, 1893, p. 137.

d'accomplir des efforts volontaires simultanés avec les deux moitiés du corps indique déjà la co-existence et par conséquent la lutte des deux actes psychiques distinctes: l'attention ne peut se porter en même temps sur les deux, mais il faut qu'elle néglige un, alternativement pour produir l'effêt maximum."

Evidently then one might infer from the foregoing that simultaneity of symmetrical impulses is but a very rapid succession of impulses with an ensuing inhibitory interference. This result might have served as a starting point for a study of the mutual influence of rapidly alternating impulses, but PATRIZI's subsequent researches diverged more and more in the direction of fatigue investigations.

JASTROW[27] in a preliminary survey on the interference of mental processes set himself the task of finding out which processes hinder and which aid one another. He used two types of processes, (a) the performance of finger movements, involving rhythm and counting, (b) processes such as adding and reading under various conditions.

The question as to how far various movements will be interfered with by the accompanying processes is answered by the conclusion that the simpler movements are less interfered with than the more complex ones. In describing the nature of interference, he says that it manifests itself in an increased effort, a great irregularity, presence of errors and a lengthening of the time of movement, from which he infers that "motor processes . . . interfere with motor ones while refraining from movement during intellectual effort would be helpful."

HANS BERGER[28] studied the rapidity with which simple finger movements could be made uninterruptedly. He found there were fluctuations in the speed that suggested periodicity in the discharge of the required impulses. The same phenomenon was observable in his preliminary experiments on the rapidity of the volitionally excited eye-lid reflex. Here the subjects had no stimuli upon which to react, the instructions being merely to

[27] JASTROW: American Journal of Psychology, vol. IV, 1891-2 p. 219 ff.

[28] BERGER: Über Periodische Schwankungen in der Schnelligkeit der Aufeinanderfolge Willkürlicher Bewegungen. Zeitschrift f. Psych., 1909. vol. L 321.

make the movement as fast as possible for a certain period of time without stopping.

BERGER's results do not throw any light on the actual influence of one impulse upon another, for one reason that the required movements in his case were homogeneous, but his general conclusion regarding the phenomena of periodicity is important and will be brought up again later in discussing our results.

DRESSLAR's[29] study of voluntary movements might be mentioned here only in so far as one of his results seems to suggest that increased central activity produces increased rapidity in voluntary movements.[30]

Many of the experiments on practice, voluntary control, and fatigue, cover some of the facts of our present problem, but from a different angle. With regard to fatigue, it is interesting to note that several of the phenomena ascribed by YOAKUM[31] to fatigue conditions, have been obtained in our investigation where every effort was made to eliminate fatigue conditions as much as possible. These results will be discussed fully under another rubric where further reference will be made to such investigations as have some points in common with our work, even if the starting-point is different.

Probably the nearest approach to our problem of interference was a series of experiments carried on in BECHTEREV's laboratory in Petrograd. As reported by BECHTEREV,[32] the purpose of these experiments was to determine the "mutual influence of personal[33] movements." The series was begun by OSSIPOFF[34] who had his subjects go through flexion movements on the ergograph

[29] DRESSLAR: Some Influences which Affect the Rapidity of Voluntary Movements. Am. J. of Psych. vol. IV, 1891-2, p. 514-527.

[30] *Ibid.*, p. 523.

[31] YOAKUM: An Experimental Study of Fatigue, Psych. Review, Monograph Supplements 46.

[32] BECHTEREV: Objektive Psychologie, p. 444.

[33] The word "personal" is really equivalent here to the word "volitional." Since Bechterev designates his system of psychology, Psycho-reflexology, he has adopted the term "personal reflex" which seems to be a makeshift for purposive or volitional activities.

[34] Ossipoff: On the Question of the Coördination of Movements. (In Russian.)

with the middle finger of the right hand, while at the same time they were to carry out, upon a given signal, other movements, either with the upper part of the body or else with the two lower extremities. The results are not striking. We learn, in the first place, that active movements of the other extremities or fingers or toes always modify the investigated movement, and that the degree of the influence depends upon the degree of muscular strain that is rquired for carrying out the investigated movement.

This investigation, however, is after all, as the title suggests, one of coördination, and judging from the brief description of BECHTEREV, it does not go behind the results so as to throw any light on the relation of the impulses involved in the interference.

SOLOVTSOFF and BARANKEYEFF[85] who took up the supplementary phase of the foregoing problem, studied the flexions and extensions of two fingers, obtaining among others the following results: The investigated movement is affected by every other movement: the more complicated this primary movement, the greater the influence. The curve of the primary movement is more markedly affected by non-rhythmical movements than by rhythmical ones. The influence becomes less marked with repetition as a result of practice. Simultaneous movements of both extremities are often of less influence than the movement of the opposite extremity. Finally it is pointed out that the mutual influence of movements presents marked individual differences.

In these two investigations the movements were carried out simultaneously, or at least, that was the task of the subjects. But in the case of simultaneous movements, we really have one impulse; hence whatever interference occurs here is not an interference of impulses, for the impulse is to make both movements at the same time. As in the case of PATRIZI, the condition of *simultaneity* can, from one point of view, be regarded only as a preliminary step. To study the influence of impulses upon

[85] Solovtsoff and Barankeyeff: On Mutual Influence of Muscular Movements. (In Russian.)

Russian names have been transliterated phonetically. The Polish names, however, retain their original spelling for the obvious reason that the Polish alphabet is, except for the accents, the same as our own.

another, we must start out with the ***succession*** of movements as a requirement.

In LANGFELD'S[36] work on simultaneous and alternating finger movements, additional light is thrown on the succession of motor impulses. LANGFELD examined the relation between the different finger movements of both the right and left hands with regard to facilitation and inhibition. The object of this investigation was different from the one which we are concerned with at present; which means naturally that the methods in each case were not the same. One main difference was that LANGFELD'S problem did not call for any presentation of stimuli. The subjects were simply instructed to react one way or another, and though the movements must be regarded as of volitional origin, the process might have gone on automatically, once a start was made. Only in the complete alternation series where the subject was not to begin moving the other finger until the first was brought to rest on the tapping board can we talk of a species of stimulation occurring, viz., auto-stimulation. Whatever interference it was that gave rise to the considerable degree of inhibition as shown in that series of experiments, especially with complete alternation, we cannot ascribe it to the same processes that would be involved in a situation where the subject is to react to a series of *presented* stimuli, simply because the conditions are missing that would give rise to those presumable processes.

The significant result in LANGFELD'S work is, for our purpose, the increasing inhibition with the increased voluntariness (complete alternation) of the tapping, and also the fact that there is no appreciable gain through practice in these movements as compared with the simultaneous tapping movements.

[36] LANGFELD: Facilitation and Inhibition of Motor Impulses. Psychol. Review, vol. XXII, 1915.

CHAPTER IV

A. Report of Experimentation

The experiments to be reported were carried on in the Harvard Laboratory during the academic years 1913-14, 1914-15. Nine subjects took part in 1913 (3 dropping out after the first term), 10 in 1914, and 12 in 1914-15. In all there were 23 subjects. These were: Prof. H. S. Langfeld (Harvard), Prof. J. W. Bridges (Ohio State), Dr. H. Burtt (Simmons), Dr. R. C. Givler (Washington), Dr. E. C. Kellogg (late at Minnesota), Prof, H. T. Moore (Minnesota), Dr. S. L. Pressey (Indiana), Dr. E. C. Tolman (Northwestern), Dr. L. T. Troland (Harvard); Miss F. Brotherton, Miss O. Martin, and Miss M. Seeley (Mrs. Givler); Messrs. Avery, Beazley, Beery, Sturges, Cutting, Davidson, Finkel, Fry, Marston, Nutter, and Sugarman. Of these the latter 7 were not college graduates at the time, but had had considerable training in psychology. Prof. Langfeld and Drs. Burtt and Troland participated as subjects during the entire work, i.e., both in the simple movement and graphic experiments. Occasionally, too, the writer himself acted as subject in order to gain more insight into the introspective accounts of the actual subjects. The records obtained in these occasional experiments, however, were not considered in the general computation.

The description of the method of procedure and apparatus in the following pages deals only with the simple movement experiments carried out during the academic year 1913-14.

B. Method of Procedure

The *apparatus* consisted of a brass cylinder, a kymograph, a screen, and two telegraph keys. The cylinder, which was run by weights, was used for exposing the stimuli, while the kymograph with smoked surface served to record the reactions of the subject.

A long series of green dots and red crosses formed the *material* for the stimuli. These symbols drawn in ink represented two different movements to be made by the subject. Each series was made up of about 85 dots and crosses, and represented one revolution on the cylinder. The period of work covered six continuous revolutions, and as the single revolution took 33 seconds (28 for the very quick reagents), it means that the subjects worked for a continuous period of 3 mins. 18 secs. There were two such periods at every sitting. In every case the subjects were allowed a sufficient rest interval between the two periods.

The rate of the revolution was constant, but the stimuli on the list were arranged at increasingly shorter intervals.[37] Thus the subjects would have to keep constantly increasing their speed in order to perform their task. The intervals between the dots and crosses ranged from ¼ of an inch to 4 mm., which gives a range of from 2 stimuli per second to 3.18 per second. In this way the subject was started with something he could do with a little effort, but which he could not continue doing for any length of time. It is obvious that to use the same rate for every subject would not satisfy the requirement of the experiment. Hence the very quick reagents who found no difficulty in performing the task in 33 seconds per revolution were given a more rapid rate (28 secs.). Each reaction to a stimulus consisted of two taps on the telegraph key, the details varying with the different variations of the experiment. A single sitting thus called for over 2,000 taps or single reactions. The keys were placed a few cm. apart so as to prevent making the two different movements in response to one stimulus.

General Arrangement. The subject sat in front of a large screen with an opening (2 × ½ cm.) large enough to expose 3 or 4 of the stimuli. A small square of mirror pasted on the back of the screen just above the opening served to illuminate the stimuli just as they would come into view. Beginning with the first variation, an arm support was used for the purpose of eliminating as much as possible fatigue and discomfort.

[37] There were a number of preliminary experiments with a somewhat different method where the stimuli appeared at equal intervals, but the rate of the revolution was changed each time.

After every one of the six revolutions of the stimuli-series there was a break where the two extremities of the paper list would meet on the metal cylinder. The break was never more than 5 mm. in length, and served as a signal for the subject to say "now." The spot was then marked by the experimenter on the smoked paper as separating one revolution from another.

At the suggestion of Dr. Troland, the following device was made for the purpose of obtaining a model or objective record with which the subjects' records could be compared. Two metal brushes were attached to a wooden square which was glued on to the back of the screen a few cm. above the opening. These brushes extended far enough so as to touch with their looped ends the stimuli-list as it was revolving on the brass cylinder. On the stimuli-list, slits were cut out above the red crosses and below the green dots. These slits formed the locus for the brushes, making an electrical contact every time the brush touched the cylinder, it being understood, of course, that the brushes were connected with two additional markers registering the "model reactions" on the smoked paper.

It was found afterwards, however, that it took less time to compare the subjects' records with the original stimuli-list than with the model record, and also that the latter method was not so safe as the former, due perhaps to technical flaws in the construction.

C. *Instructions*

The instructions to the subjects were both general and particular, the former having reference to the experiments as a whole; the latter, to the separate variations.

The general instructions to the subjects were to consider the task as if it had to be done *in toto*, but should they find it impossible to meet the requirements, they were to use their own judgment. They were not told to lay more emphasis on accuracy or speed or some one specific factor. Such an instruction might easily mar the results; for those very individual differences that the various types of reaction would reveal in our work are of

paramount importance.[38] Secondly, cast-iron rules would, in this case, be not only unnecessary, but a decided drawback, as the particular instruction might be a help to one subject, while to another it might act as a disturbing factor.

In the first set of experiments the particular instructions were to react to the red crosses by making a double up-down movement; to the green dots, by making a double centripetal* side-movement on the telegraph key, the latter being suitably arranged in a vertical position 3 cm. above and 5 cm. to the right of the key intended for the up-down movement. The forefinger of the left hand was to be used exclusively for the tapping of the key, but the side reactions necessitated the turning of the hand and the use of the forearm muscles.

D. *Variations*

The combination of stimuli varied from week to week. There were also several variations in the conditions of the experiment. In the first variation, the reactions were double up-down movements with the forefinger and the middle finger. The second variation called for the use of the middle finger and the ring finger; while lastly the index finger was again to carry out both movements. In the one, the key was hit with the tip; in the other reaction, the nail part of the finger was used. The reactions here were virtually flexor and extensor movements (downward and upward).

[38] "When a person is given vague and indefinite instructions, he very soon helps himself out by adopting a stereotyped method and sticking to it, i.e., he adds self-imposed tasks to the one he has been set" (Washburn: Movement and Mental Imagery, p. 172-173).

It is not advisable to give such vague and indefinite instructions that would allow the subject to interpret his task with too great a latitude, but in experiments where individual differences are sought, it would be just as wrong to suggest in the instructions some one definite type of reaction as to do the exact opposite.

Cf. also Stern's injunctions: "Fragt er [the subject] etwa ob er seht schnell oder langsam, stark oder schwach klopfen solle, so muss die immer zu wiederholende Antwort lauten 'ganz wie Ihnen gefällt.'" W. Stern: Ueber Psych. der Individuellen Differenzen, p. 117.

* i.e., adductive or towards the median line.

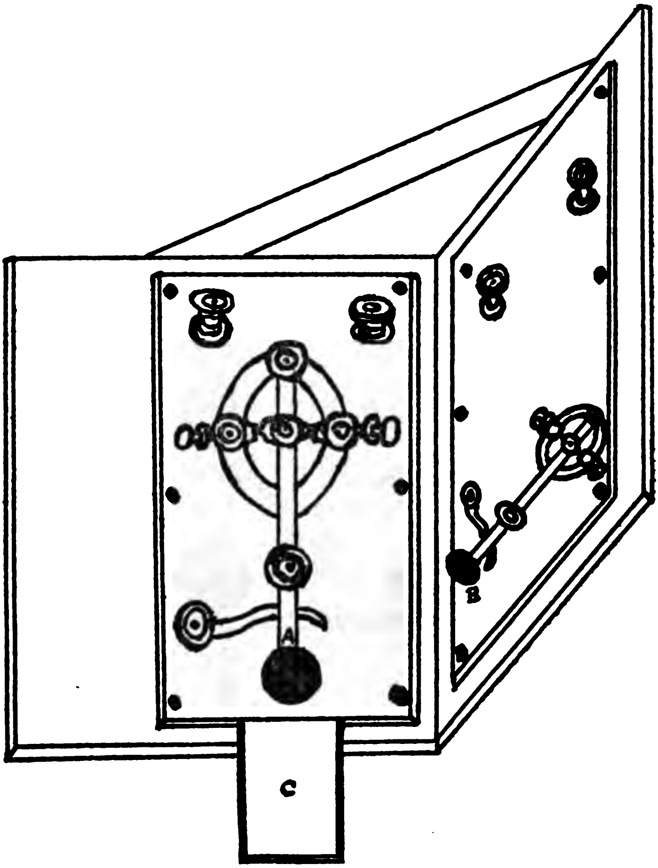

FIG. 1.—*Diagram showing position of keys for series* 1.
A. Horizontal key requiring downward movement.
B. Key in upright position for lateral movement towards the median line (adductive).
C. Arm-rest.

In all variations, care was taken to avoid disturbances due to confusion arising from reversed instructions. Only one change was introduced at a time, so that the association of stimulus and reaction became more or less stereotyped.

Of the twelve revolutions of the stimuli-list given at a sitting, six began with the closely arranged dots and crosses and ended with those farther apart, while the other six revolutions were

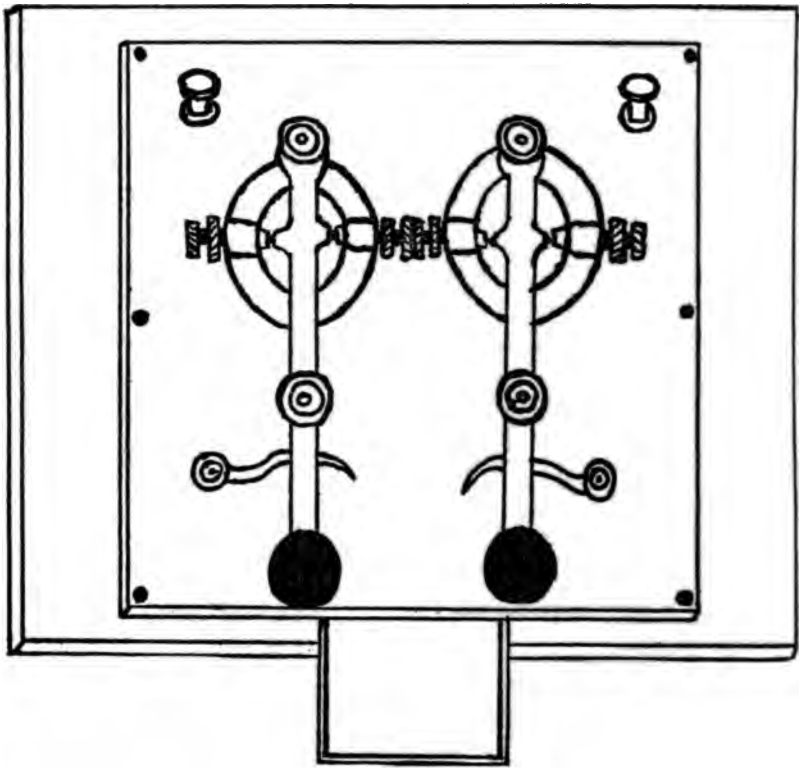

FIG. 2.—*Position of keys for Series II and III (variation 1 and 2).*

of the same list, but reversed, i.e., they began with the stimuli farther apart. This order of the lists was varied in alternate weeks. As far as the subjects were concerned, it was not one list, but two entirely different lists which they designated respectively "fast first" and "slow first" referring to the speed corresponding to the different parts of the series.

E. 'Spontaneous' Record

Towards the close of the simple movement experiments, the subjects were given a slightly different task. They were asked to go through the same kinds of movements as before, but without any stimuli before them. They were simply to react on the basis of their impression of previous arrangements, or rather of

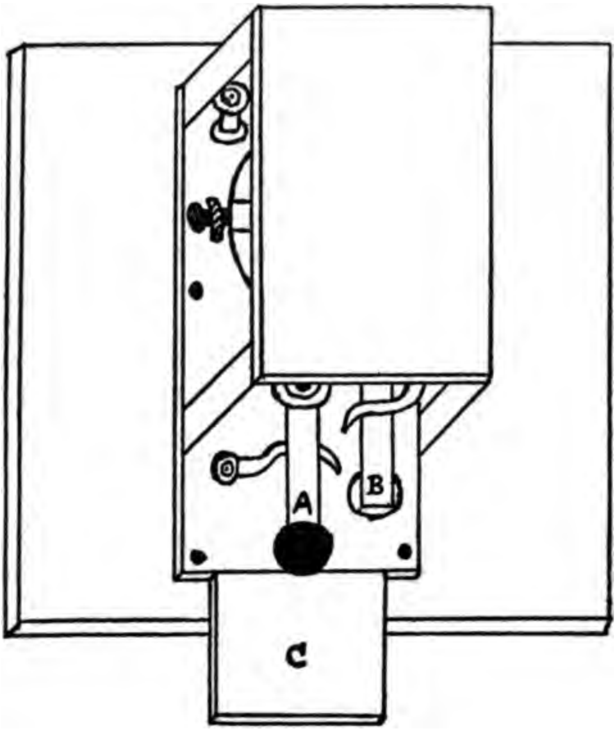

FIG. 3.—*Position of keys for series IV (variation 3).*
Horizontal key struck with palm or side (tip) of forefinger (flexor ement).
Key facing A struck with dorsal side (nail part) of forefinger (exten- novement).
Arm-rest.

'feel' of the general rhythm in former experiments. This ht be called a 'spontaneous reaction' test, the purpose of ch was to compare the subjects' spontaneous reactions with se in response to a strained and constantly changing situation. ll other respects the situation was the same, the record on the m being divided into six equal parts of 30 secs. each so as to esent the six revolutions of the stimuli-list. The movements e as in the third variation (forefinger flexor and extensor ements).

F. Tests as to Difficulty

fore evaluating the results it was necessary to ascertain the tive difficulty of the different reactions. We may easily

suppose that an up-down movement, such as is done in ordinary tapping, is much easier to perform than a centripetal side-movement. All the subjects spoke of the latter as being by far the more difficult of the two. To rely, however, on the statement of subjective difficulty by the subjects would be an assumption which might not be borne out objectively. For this reason, short tests (1 min. and 30 secs.) were given with every variation, where the subjects were required to go through one reaction continuously as rapidly as possible until the signal was given to stop. The same was done with the second reaction. By counting up the actual number of single continuous reactions of the two different movements, the writer was able to compare with definiteness their motor difficulty.

G. *Auxiliary Experiments*

In order to get at the conscious content during critical moments and also to clear up some of the data appearing again and again in the protocol, a number of experiments were given for the sake of introspection alone. There are very few problems in which a minute analysis of the introspective content counts for so much as in the phenomena dealing with the higher thought processes and the neighboring problems. It is just here, however, paradoxical as it may seem, that "thought" seems to elude us most. Especially is this true in our present work. The subject who is straining every muscle to accomplish the physically impossible, so far as he is concerned, cannot be expected to hold in mind the intricate details of his experiences during the 3.3 mins. that he is at work. Repetition, as has often been suggested, will bring out, doubtless, more and more of the elusive material. ACH,[39] it will be remembered, relied wholly on perseverative tendencies for introspection. This confidence in the recurrence of the memory image is, however, not shared by G. E. MÜLLER.[40] It is agreed on all hands that introspec-

[39] N. ACH: Willenstätigkeit und das Denken p. 19 ff. In his later work "Ueber den Willensakt und das Temperament" the general methodological view is the same.

[40] G. E. MÜLLER: Zt. f. Psych. Ergänzungsband V, 1911, p. 122: Zur

tion is at bottom retrospection, but it makes quite a difference as to what degree of retrospection it is; in other words, the value of the introspective account will depend largely on the period of time elapsing between the actual experience and the reproduction by the subject. Hence, the sooner we can exploit the subject's material the more certain we are of our find.

It was in accordance with this principle that the additional experiments had been introduced. The purpose was to catch the conscious experiences in the process of congelation. Instead of waiting till the working period was finished, the introspection was given at the crucial moment.

This auxiliary method was carried out in two ways: In the first place, the subjects were asked to stop their work as soon as they noticed anything striking (mistakes, confusion, definite inhibition, etc.) and to describe their experience at that point as closely as possible. Or they were told that at a certain stage in their work, the cylinder presenting the stimuli would stop revolving. At that point they were to reproduce on the instant their whole content of consciousness with its antecedents. In the latter case, i.e., whenever the break was conditioned by the experimenter, the machine was usually stopped while the subject was reacting to the closely arranged dots (highest speed). These auxiliary or control experiments were given towards the end of the hour, after the subjects had already made two kymographic records, and had therefore ample opportunity for introspection in the earlier part of the hour.

H. *The Questioning*

The questioning of the subjects is another source of perplexity. How much is to be asked, and how exacting ought the experimenter to be in taking introspection? Should the sub-

Analyse der Gedächtnistätigkeit und des Vorstellungsverlaufes, pp. 139-143. Müller is quite emphatic in his disapproval of Ach's method. "Ich würde eine Versuchsperson wegen offenbarer Flunkerei mit Schimpf und Schande von dannen jagen, wenn sie mir erklären würde, dass sie alle oder wenigstens viele der dort angeführten Fragen beantworten könne, indem sie ihre Aufmerksamkeit bald dem einen bald dem anderen Teile des perseverierenden Inhaltes zuwende" loc. cit. p. 140-141.

ject be closely and "systematically" questioned, as ACH was inclined to believe all through his work, or should the call for introspection be general and under no circumstances insistent?

The writer followed a midway course. Probably most psychologists would approve of G. E. MÜLLER's caution not to overwhelm the subject with a volley of questions;[41] and the criticism levelled at ACH to the effect that the subject is liable to believe that the score or so of questions must be definitely answered—is not without point. Yet, on the other hand, it must be pointed out that unless there is some guiding control, the introspection is so scant and often so irrelevant that it is scarcely worth while recording. In our particular case, for instance, though most of the subjects were trained observers, they were inclined to content themselves in the early stage of the experiment with stating such perfectly obvious facts as "Had to omit because it was too fast," "The one movement conflicted with the other," etc. Now it is clear that such statements cannot contribute to our knowledge regarding the mental processes responsible for these very omissions and other resulting phenomena. The subject must be given some idea as to the dimensions of his task. He must at least be given to understand that his *Aufgabe* includes introspection as well as going through the actual work of the experiment. So many are prone to believe that their task merely consists in performing the required reaction, and that introspection is at most a by-product—something that goes into the bargain. While this attitude is the very thing advocated, and in its extremest form at that, in certain radical quarters, and is to a certain extent allowable in quite a number of experiments, it certainly cannot take us very far in such a problem as the one before us.

What the writer did in the way of guiding the subject was to ask him to illuminate his introspection by further analysis. When, for example, the term 'conflict' or 'inhibition' occurred in the protocol, the subject was requested to describe the experience more fully. As a rule, the effect of this exhortation was noticeable only in subsequent sittings, but not at the time the

[41] G. E. Müller, loc. cit. p. 122.

questions were first put. The significance of the introspective task seemed to grow on most of the subjects. Those, however, who proceeded no farther than to say what could easily be told by consulting their kymographic records, were never urged to give more details, except in explaining the terms employed by them. Fortunately it may be said that ACH's finding "Vor allem scheinen gut geschulte Psychologen nicht besonders brauchbar, da sie zu vorsichtig sind und infolgedessen andersartige Determinationen nicht wirksam sein können"[42] was not confirmed here. It was decidedly the more advanced subjects who showed better orientation in their introspective situation.

I. Arrangement of Results

Before concluding this chapter, a few words may be in order relative to the general arrangement of the results in this study. The results are treated under three separate heads: (a) those based on the kymographic records, (b) those based on the protocols, (c) individual differences. The correlation will be evident from occasional cross-references and also from the synthesis of the results.

[42] Ber. über den II Kongress f. Experim. Psychol. 1907. p. 256 cited also in his reply to Selz "Willenshandlung und Temperament," Ztschft. f. Psych. vol. 58, 1911.

CHAPTER V.

Objective Results.

The objective results of the simple movements are based on over 500 kymographic records or about half a million single bits of data. In computing the results there were, of course, one or two drawbacks that no conceivable technique could remedy, but the introspective account filled out some of the gaps.

The task of the writer was naturally to compare the original stimuli-list with the actual work done by the subject as revealed by the records.

A. Method of Computation.

Since it was possible for the subject to go wrong in four different ways, *viz.,* (a) to give single reactions instead of double ones (see instructions), (b) to omit the entire reaction, i.e., the double tap, (c) to *add* single reactions, (d) to add double reactions—the figures were arranged in 8 columns, i.e., 4 columns for each of the two movements.

The double reaction representing a single stimulus is the unit. Single reactions were marked ½. The plus sign denotes that the movement has been added, with no stimulus to correspond to it; the minus sign is used for omissions. Each revolution of the stimuli-list was computed separately, so that the course of the work could be followed from revolution to revolution.

As the tallying proceeded, other phenomena besides omissions and additions were noticeable on the records. It was possible to tell long inhibitions, rhythmic activity, and various other things that will be described presently. Unfortunately it was very difficult indeed to spot mistakes, i.e., wrong reactions. Tracts of utter confusion could be detected much more easily.

B. General Results.

The inhibitory effect of one impulse upon another is the most marked, though by no means the most instructive, phenomenon

in connection with the entire mass of data. We can now amend the statement cited from LADD and WOODWORTH's Physiological Psychology to read as follows: When a series of dissimilar acts must be performed in quick succession, they are bound to *exercise an inhibitory effect upon one another*. But it may be added here that they affect one another unequally; and this leads us to examine our previous formulation of the problem anew.

As stated at the outset, the question was one of how we go about our task when we have two *different* kinds of impulses following one another in quick succession. But what constitutes their difference? To be sure, the different reaction to be performed according to the instructions governs the impulse. The *mere difference,* however, can have no significance for the reagent, except it be expressible in comparative terms. In other words, what is it that would determine the subject to act one way or another? The only basis that suggests itself as the determining factor is that of pleasantness and unpleasantness, or, taking it one step farther, that of *ease* and *difficulty,* taken either subjectively or objectively;[43] for in the conditions of the experiment, there was nothing else likely to act as a basis for the production of any marked feeling-tone, unless we assumed an extraordinary aesthetic discernment on the part of the subjects, something which was not brought out in any of the protocols. The difference in the color and shape of the stimuli, as well as the slight difference in the sound of the two keys, served only as guides and not as aesthetic factors.

We are now in a position to isolate and specify one phase of the problem of interference, and ask ourselves: Given a series of two sorts of impulses, the one governing a more difficult movement, the other an easier movement; which of the two movements will predominate?

The answer is, and this emphatically, that the easier movement of the two will be favored.[44] There are only a few isolated

[43] It is important to distinguish between the degree of ease or difficulty as *supposed* by the subject (subjective) and the actual degree as revealed by the objective tests (objective).

[44] It will be found in the second part of this investigation that this result does not hold universally of the complex graphic movements. That difference and the grounds upon which it can be explained are significant.

instances in which this did not occur—not more than a dozen revolutions out of nearly 3,000 revolutions—and even these sporadic cases were all but one or two explained in the introspective protocol. (One subject, LV for instance, explained, at the time, he had green after-images with the green dots; and as the green dots happened to represent the more difficult movements, he, on several occasions, emphasized the more difficult movement. Subject ML spoke once of putting most of his attention on the green stimuli. Another explanation of the seeming discrepancy lay in the fact that the subject wrongly associated the movement and would keep on making one reaction instead of the other throughout the revolution without becoming aware of his confusion).

The mutual, inhibitory effect was universal, and in the main manifested itself in omissions and confusion. The task was abbreviated, the more difficult movements suffering more than the easier. In subject LV, the interference did not give rise to omissions so much as to the execution of additional movements performed haphazardly. This anomaly will be discussed under individual differences.

Taking our first set of experiments, in which the double-down tap was to be compared with the double-side tap, we can point to the following tables:

TABLE I.

Totals for All Subjects Taken Collectively.

Type of Movement	OMISSIONS		ADDITIONS		No. of requisite reactions	No. of stimuli
	Double	Single	Double	Single		
Vertical	8.8%	5%	.55%	1.65%	40,542	20,271
Lateral	12.88%	7.06%	.37%	.92%	41,598	20,799

The difference between the results in the respective movements is striking in every respect. Not only is the emphasis on the vertical movement shown by the fewer omissions and less resorting to single reactions, which would indicate of course a shirking of half the work, but it is also noticeable in the tendency to add

more vertical movements than lateral movements. We may say then that the favoring of the vertical movement manifests itself both in omitting fewer and adding more reactions in this type of movement than in the lateral. The additions are, of course, mistakes, but mistakes that reveal the dominant tendency to favor the one movement instead of the other.

The results of the individual subjects with regard to these two movements are even more clear-cut, for it will be seen that we can scarcely talk of any exceptions to the rule of omissions. Whatever exceptions do appear, are to be found only in several isolated cases, most of which are explained in the introspection (vide supra, p. 34). As for the tendency to add more vertical movements than lateral ones, subject RU is the only exception, but it must be explained that in this series of experiments the rate of speed suited him much more than the other subjects, and the task (both the vertical and lateral movements) for him was easy. Later, however, in the subsequent variations, the subjects were classified as to speed, and he was given a shorter period, which means, of course, a higher rate.

C. Translation of the Results.

These tables are not intended to prove merely that pressing a telegraph key *downwards* is a more favored movement than pressing it *sidewise,* nor should we be satisfied with drawing the conclusion that a downward movement is more easily made than a lateral movement. We rather set out with this conclusion, and though no tests were given to compare the technical difficulty of the vertical and lateral movements (the tests were started with the first variation), it would be easy to cite the results of such a comparison from the literature, even if we should feel disposed to discount the emphatic statements of the subjects to the effect that the lateral movement was more difficult than the vertical movement; and in this there was no difference of opinion, except as to the degree of difficulty. In experiments where the ring finger was used, J. C. BARNES found that "the lateral movement of the ring finger presented more

TABLE II.

Results for Individual Subjects in Per Cent.

Subject	Type of Movement	OMISSIONS		ADDITIONS	
		Double	Single	Double	Single
BU	Vertical	1.28	2.56	.68	.94
	Lateral	1.77	2.4	1.3	1.3
RO	Vertical	.87	3.6	.1	.37
	Lateral	2.02	6.2	.067	.3
FG	Vertical	3.46	7.07	.66	2.98
	Lateral	4.5	12.45	.125	2.16
GD	Vertical	7.08	2.34	1.45	2.74
	Lateral	13.43	5.45	.773	1.51
LE	Vertical	3.	7.5	1.07	4.9
	Lateral	15.2	13.9	.305	.915
ML	Vertical	6.95	9.15	.716	2.94
	Lateral	12.9	12.63	.4	1.38
TR	Vertical	16.12	1.18+	.04	.084
	Lateral	18.12	1.17+	.079	.04
TT	Vertical	13.18	15.6	.19	2.2
	Lateral	26.35	18.67	.076	.53
SR	Vertical	27.3	2.61	.168	*
	Lateral	30.85	1.07	*	*
		AVERAGE			
	Vertical	8.8	5.73	.56	1.9
	Lateral	13.9	8.21	.347	.903

difficulties than the vertical"[45] and points out rightly that the movement in the lateral direction is not so common as that in the vertical direction. This would hold especially for the forefinger which is so frequently used vertically in every-day activities—very rarely only in the horizontal plane.

The shunting of the difficult movement in favor of the easier one was not due to any perfunctory attitude on the part of the subjects. The writer who was at first inclined to suspect that not all subjects were "doing their best" soon found by actual trial that he had done the same thing, viz., emphasized the easier movement at the expense of the more difficult one.

It is interesting to compare our results with those of BAUCH,

[45] J. C. Barnes: Voluntary Isolation of Control in a Natural Muscle Group. *Psychological Review, Monograph Supplement* 93, p. 19.

* Subject SR showed no tendency to make additional movements. Only four such reactions were found in this series of records, and they were all on the vertical side.

who had his subjects move their forefinger with the greatest possible speed from one point to any other of six points arranged in the form of a hexagon. He found that the most convenient (*bequemste*) movements were the most frequent,[46] and also that the most frequent movements were the most rapid.[47] In fact, most of his results point in one direction, viz., that the easier movement is the one likely to be favored as against the more difficult one. This one conclusion really forms the essence of most of his results when we reduce these to the lowest terms; for all his types of movements (centripetal and centrifugal, flexor and extensor, towards the median plane and away from it) resolve themselves into the more difficult and the easier type of reaction. Similarly KRAMER and MOSKIEWICZ are inclined to connect the slower with the more difficult movement and the easier with the more rapid movement.[48]

These and other results of an analogous nature must not remain detached facts. They should be synthesized and studied in relation to one another, for they form a chain of results out of which it is possible to deduce or verify a fundamental principle that would possibly explain a number of phenomena hitherto observed in a more or less casual way.

D. *The Course of Least Exertion.*

The outstanding feature of the results both in the simple movement and the graphic experiments is the *universal tendency to move along the lines of least resistance.* So universal is this tendency and so numerous and far-reaching its manifestations that it would scarcely be presumptuous to formulate it into a law, once it is shown to operate in connection with other similar problems. For the present, however, let us content ourselves with regarding it as a *determining tendency,* but a determining tendency of a special kind.

The term "determining tendency" which has been in vogue

[46] M. Bauch: Zur Gleichförmigkeit der Willenshandlungen in Fortschritte der Psychologie II, 1914, p. 367.

[47] *loc. cit.*, p. 354.

[48] Kramer and Moskiewicz: Beiträge zur Lehre von den Lage und Bewegungsempfindungen in Zt. für Psych. vol. 25, 1901, p. 121.

since Ach published his experimental results on the will has come to connote a certain definite thing in psychology, and it would be quite unnecessary to substitute another term for it. But in reviewing our results, it becomes evident that we must distinguish between different kinds of determining tendencies which Ach probably found no occasion to do.

The determining tendencies that came up in Ach's work were those directly connected with the *Aufgabe*. We might expect different determining tendencies at work with different tasks. We should therefore be inclined to consider these as *subsidiary* determining tendencies depending upon some fundamental factor that is at the root of the several tendencies which it governs. This more fundamental director may be called then the *primary* or *dominant*[49] determining tendency. The *individual* determining tendencies responsible for individual differences we propose to call *"personal determinants,"* because they constitute collectively the complex known as personality.

Setting out with our primary determining tendency, viz., the course of least exertion, we shall see that all the other tendencies are either manifestations of the same principle or else modified by it.

The course of least exertion has been taken account of in more than one work, but so far as the writer is aware, it has not been treated experimentally, though it is possible to refer back some of the results obtained by Sherrington and other British physiologists to this very principle. Some of these references will be cited later.

As early as 1894, the brilliant historian Ferrero tried to build up a sociological theory on the conclusion that "This law of least effort regulates the psychical activity of man."[50] This conclusion, in his case, is based on very thin evidence, often on bare

[49] Titchener in describing the experiments of Külpe on abstraction uses the word "instinctively" in a peculiar connection when he says: "When no preliminary directions were given, the observers instinctively set themselves the easier tasks" (Text-Book of Psychology, p. 530). In view of that, our term "dominant determining tendency" should not be considered as too emphatic an expression of the fact.

[50] Guglielmo Ferrero: Les Lois Psychologiques du Symbolisme, p. 18.

assumptions and sweeping generalizations; yet we cannot afford on that account to lose sight of the suggestive idea that FERRERO has hit upon.

In his last work[51] the late Professor Ribot has approached the subject with more insight than his younger contemporary, applying the "least effort" tendency in various connections in close conjunction with the law of parsimony or economy. The pith of his remarks is expressed in the following:

"Entre les deux, (i.e., the tendency towards the least effort and the law of economy) il y a un fond commun et identité de nature. On pourrait se risquer à dire que la tendance au moindre effort est un genre dont la loi d'économie est une espèce ou une variété. Ce qui les différencie c'est ceci: La tendance au moindre effort en général a sa fin en elle même, son idéal est le repos et ses résultats sont négatifs; la loi d'économie est un moyen pour la simplification au travail et ses résultats sont positifs."[52]

Now whatever the relation be between the law of economy and the tendency towards the least effort,[53] whether the former be a biological explanation of the latter or not, one thing seems pretty certain, and that is: a great many motor phenomena hitherto studied and reported as desultory facts should be regarded in the light of and referred back to that very tendency.

In our particular work there are scarcely any phenomena which could not be brought in line with, if not actually explained by, the course of least exertion. Thus it manifested itself in various forms. One of these was the tendency of the subjects to start in again on the easier reaction after a break in the work caused by the difficulty of keeping up with the rate—a result which is of great significance in educational psychology. Unfortunately, no figures are available for this result. The subjects were evidently unaware of this fact for some time; and in order to record all the cases where the subjects "came to" on the easier movement, it would have been necessary to take into account several side factors, so that the already great burden would have been doubled.

[51] Th. Ribot: La Vie Inconsciente et les Mouvements.

[52] *loc. cit.*, p. 131-132.

[53] It seems more desirable to use the term "exertion" instead of "effort" which is sometimes employed in the sense of a feeling and is, at all events not so objective a concept as exerti...

Suffice it to say that, in checking up some of the records occasionally with this purpose in view, the introspection on this point was confirmed: immediately after a gap in the record, the reaction for the easier movement followed.

The Levelling Process.

Closely allied with the foregoing is a group of phenomena that seem to point to a *compensatory* or equalizing process. It had been noticed during the computation of the records that omissions were followed on many occasions by additions, and *vice versa*. At first this was thought to be only accidental, at least not fraught with any special significance, for there is no reason to suppose that the subjects are immune from making mistakes in the number of taps, even in places where they could keep up with the stimuli. Thus instead of making, let us say, six reactions for the three green dots and four reactions for the two red crosses, they might make five each. In a case like this, one would be likely to suppose that we are dealing here merely with a rhythm phenomenon; and to be sure, rhythm plays no mean part in the interpretation of our experimental data, and instantly suggests itself as a form of expression of that which has been called here the primary determining tendency. But the compensation mistakes were not predominantly of this kind. In the first place, most of these occurred not within an immediate sequence of alternate stimuli, but within a sequence involving the *same kind of reaction,* in other words, an omission in the red was made up for by an addition in the following red, and likewise with the green. Between the two groups, however, the intervening group of the alternate color is very likely to have broken up the rhythm, supposing that this might have caused the repetition of the same number of taps. Secondly, by far the largest number of compensatory reactions did not involve an equal number of reactions. Thus frequently the relation was such as 5 and 3 instead of 6 and 2, or 4 and 3 instead of 2 and 4 reactions. In some cases the compensatory reactions took the form of simple *transpositions* as 4 and 2 instead of 2 and 4.

All these cases of compensations were marked on the records

and counted up, in a separate column, beside the omissions and additions; and though their ratio is very small in general, it seemed altogether too striking to be disregarded.[54] It must be remembered also that in considering the ratio of such compensations, we must not compare them with the total number of reactions or even stimuli, but with the number of groups of stimuli. Under such circumstances, 6 or 7 cases of compensation in a record of 200 groups (about 500 stimuli), in the average, would suggest, especially when 75 per cent of the records show the occurrence of such "give and take," that we are dealing with a genuine phenomenon and not the result of mere chance (vide appendicem). If there was any doubt of it, the results in the writing experiments dispelled it entirely. There we have the unmistakable appearance of this phenomenon in a palpable form, as might be expected in the case of qualitative data. And for this reason, i.e., in order to subsume both classes of compensation under one general rubric, the term *levelling process* has been introduced.

Something analogous is reported in ACH's work.[55] In altering his instructions to the subjects after they had become accustomed to the previous instructions, it was found that frequently the subjects could not check themselves in time and gave the wrong reaction with the result that the succeeding reaction or reactions were bound to be correct, the failure producing

[54] JUDD, MCALLISTER and STEELE (*Psychological Review, Monograph Supplements,* vol. VII. Analysis of Reaction Movements, 1905-6, p. 158) obtained something of this sort in simple reaction experiments, which they ascribe to rhythm of nervous action. "The impression is made upon one of a positive movement followed by a relaxation which in turn gives way to a rather sudden beginning of muscular contraction. The length of a single wave (from one crest to the next) is about 150 sigmas . . . These figures show that the wavy form of the line is not due to any gross mechanical causes, such as the subject's respiration. The waves represent a rhythmical unsteadiness of the subject's muscular tension."

It is for this very reason that we should regard them as connected with a compensatory tendency rather than with the periodic waves to be taken up in the next section.

[55] Ach: Ueber den Willensakt und das Temperament, pp. 272-274, pp. 51-53 and p. 49.

greater strength of determination than otherwise.[56] In the actual result, i.e., the reaction of the correct (rhyming) syllable, the condition of the experiment naturally precludes a compensatory addition, such as our results indicate; but the greater effectiveness of the determination on the part of the subjects due to the previous failure may be evidenced by the lengthening of the reaction-time as well as by the introspective experience of the reagents.[57]

Curiously enough, in our experiments, the subjects were not aware of either making up for a previous failure or relaxing after an exaggerated reaction. At least the introspective protocols, with one solitary exception, do not reveal such knowledge. What is developed here is based wholly on the objective records.

The phenomenon of compensation seems to have as its antecedents not only the interference of the impulses corresponding to the different stimuli, but also a *conflict of* two determining tendencies. The primary determining tendency, we shall remember, is the course of least exertion. If this tendency had its own way with nothing to oppose it, we should expect to find a large number of omissions, but no additional reactions. But the determining tendency of carrying out the instructions to the very limit of one's ability is a hindering factor, which at times gains the upper hand and causes the subject to make a greater effort. In fact, it is not too much to say that even the making of errors is due to this very conflict. Were the primary tendency alone in operation, mistakes would be rare. Without putting on

[56] In Mellone and Drummond's Elements of Psychology (p. 171) we find an acute observation that takes the whole principle one step further than its scope in Ach's investigation. It is now applied to every-day life where "yielding . . . is sometimes more strengthening to character than resisting; for along with resistance there sometimes goes a dwelling on the pleasure we have denied ourselves which actually strengthens its hold over us and renders us more likely to succumb on the next occasion." The apt illustrations that are adduced in evidence of this conclusion need only be supplemented by the memories of our own experiences in this matter.

[57] The writer can also report that as a subject in reaction experiments, he has frequently noticed that an unusually long reaction-time will be followed immediately by a "keying up" which, it may be safely assumed, causes the next reaction to be unusually quick.

any special effort, the subject might react to only a very limited number of the stimuli and *react to those correctly.* With a certain type of subject, (the omitters, vide infra under individual differences), such was actually the case to a greater extent than with the other reagents. What really was responsible for the confusion was the seriousness with which the subjects accepted the exacting demand of the *Aufgabe* which, whether present in consciousness or not, undoubtedly determined the attitude of the subjects.

Another phenomenon that probably comes under the same head is a tendency on the part of the quick reagents to make additional taps while waiting for the next stimulus to appear. From the introspection, it would seem that these repetitions are not anticipatory of the next stimulus. Anticipations arise frequently when the rate is far too fast, but when the rate is too slow, i.e., when the dots are far apart, the quick reagents are apt to make mistakes by being too generous in their reactions. If the group consists of a single stimulus, it may be repeated; if there are two or three in the group, a couple more reactions than necessary might be tacked on. Now it is difficult to say whether the additions in the slow part are a sort of atonement for the many omissions in the more accelerated portion of the work, or whether it belongs to *the tendency to continue the same process* which was recorded so frequently in the introspective protocols. Should the cause of these additional taps lie in the latter tendency, then it is clear that we have in this phenomenon a noteworthy instance of inertia. But whereas we can readily understand why continuing the same movement is objectively preferable to making a new movement, it is not so easy to explain why the subjects should keep on adding unnecessary taps instead of resting the fingers until the next stimulus would appear. As there were only four subjects for whom *any* part of the task was so easy that they could afford to wait here and there during the slow parts (farther apart stimuli) for the next stimulus, the data are of course very scant, but even these comparatively few cases warrant the conclusion that *for every individual reagent there is an optimal rate above which and below which he is apt*

to make mistakes—in the latter case, both mistakes of commission and mistakes of omission; in the former case, mistakes of commission alone. In other words, make the task too easy and you are putting a stumbling-block in the way of the reagent; for from the point of view of accuracy, it is just as wrong to make six reactions, as it is to make only two reactions, instead of four.

Rhythmic Activity.

Under *rhythm* we have a group of phenomena, which, though not all occurring with the same subject, appear separately in the records of the various subjects and point to some underlying factor that determines the reactions of different subjects in different ways.

That this should be the case is not surprising, when we consider to what an extent the working of the physiological mechanism is dominated by rhythm. Indeed the phenomenon of rhythm[58] and periodicity is so universal that it has been incorporated as a vital element in many an all-embracing theory of the universe. The influence of rhythm on work has been studied extensively since the appearance of Buecher's "Arbeit und Rhythmus," and recent physiological investigations (Piper, J. Loeb, Berger, Sherrington, Graham Brown, Martin, and others) have only served to emphasize the rôle of rhythm in our psychophysical make-up.

In our own work, the rhythmic tendency expressed itself (a) in a seemingly automatic repetition of previous reactions (3 or 4 groups in succession with the same number of reactions), (b)

[58] The writer is in full agreement with M. Keiver Smith (*Rhythmus und Arbeit* in *Philosophische Studien*, 1900, Vol. XVI, p. 300) who insists on the distinction between rhythm and periodicity. The distinction, however, does not rest on the difference between "bewusste Zustände' and "unbewusste Verhältnisse," or even between the organic and the inorganic world' as she seems to suppose, but rather depends on the time interval between the occurrences. Thus, pulse beats are a rhythmic phenomenon, while the menses are periodic occurrences. We must not identify rhythm with the perception of rhythm, though we have to depend on the latter for our criterion of a rhythmic phenomenon. Periodicity, on the other hand, cannot be told except through some external indications or by calculation.

repetition of the same mistakes in succeeding revolutions, (c) creating a spontaneous rhythm regardless of the stimuli, (d) stopping on odd number of reactions (it will be recalled that every stimulus represented two reactions on the key). These well-marked phenomena, apart from the introspective accounts which will be given in the next chapter, cannot but call for an explanation only on the basis of rhythm.

The entire mechanism of rhythm has not been adequately explained. *Féré* who has made an interesting contribution to the psychophysics of work, simply explains the fact by saying that rhythmic activities involve less effort.

"La répetition de l'acte volontaire dans les mêmes conditions le rend plus facile á exécuter en atténuant l'effort nécessaire par le travail mental. C'est cette atténuation de l'effort intellectuel réalisé par l'habitude qui est la base physiologique de la routine si difficile à déraciner.

"Les groupements simples des mouvements, qui en favorisent la représentation préalable, augmentent le travail."[58a]

This sounds as if rhythmic activity were one mode of realizing t tendency towards the least exertion. We can understand :ordingly in this light the finding of Trèves that in work on the ergograph, there will establish itself a rhythm representing the maximal frequency compatible with the production of constant work.[59]

Such observations,[60] afford us the *raison d'être* of rhythmic activity, but do not supply the information we are seeking, viz., the *genesis* of rhythm.

Miss Keiver Smith claims that the origin of rhythm is physiological, but the application of it to the regulation of work involves a psychical influence, for it is a means of renewing the ll impulse and the attention.[61] This again is a statement re-

[58a] Ch. Féré: Travail et Plaisir, p. p2.

[59] Z. Trèves, Sur les conditions qui déterminent le rhythme spontané dans e travail ergographique volontaire. 5 me. Congrès International de Physio' ›gie, 1901, cited by Ch. Féré *loc. cit.*, p. 20.

[60] *Cf.* also, Davis who cites two other investigations in connection with his ›wn findings of rhythm in tapping, Reserches in Cross--Education n Studie, Yale Psychological Laboratory, p. 15.

[61] M. Keiver Smith: Rhythmus und Arbeit, p. 394.

garding rhythm as a condition and not as an effect. Possibly flagging of attention has something to do with the repetition of mistakes at certain intervals, but in does not explain the automatic repetition of the number of previous reactions, nor does it throw any light on the occurrence of such a series as 5, 3, 1, 3, 5, 7 instead of 6, 4, 2, 2, 3.

It should be said also that all the reagents were subject to one form of rhythm or another. To note the most typical instances, subjects T R, L Z, S R never showed a tendency to make single reactions, and very rarely repeated previous mistakes, but they, more than any others, perceived the stimuli as groups and ignored the single dots or crosses. LE, on the other hand, perceived the stimuli separately, but followed out a certain rhythmic plan in reaction, favoring the odd number. G D was free from the latter tendency, but would repeat mistakes occasionally. R U did not show any marked development of the other tendencies, but in some parts of his work, he lapsed into a rhythm that he had picked up spontaneously, and that, he thought, had been suggested by his work in former weeks.

It may be that these facts have only a remote bearing upon one another, that the absence of rhythm in one form is not coupled with its appearance in another form, it may even be doubted whether the various phenomena described as rhythmical issue from a common root—that they are suggestive, however, of a rhythmic process which is dependent on the conjuncture of determining tendencies including personal determinants can scarcely be denied. This, of course, does not necessarily mean that the rhythmic processes are due to the conditions of our experiments. After the evidence that has been accumulating on all sorts of physiological and psychophysical rhythms from the contractions of the muscle fibres of Medusa,[62] and isolated strips of heart muscle,[63] to the most concentrated type of attention, it appears quite certain that any simple task,[64] would be attended

[62] J. Loeb: Comparative Physiology of the Brain, p. 16 ff.

[63] E. G. Martin: Amer. Journal of Physiology, vol. II, 1904, p. 103 ff.

[64] For rhythm in reading, cf. W. F. Dearborn: Psychology of Reading, p. 118 (Archives of Phil. Psych. and Scientific Methods, no. 4) and Huey: The Psychology and Pedagogy of Reading, p. 175.

by some rhythm phenomenon, as TRÈVES has, as a matter of fact, demonstrated some years ago (*vide supra*). Nevertheless, it is quite safe to surmise that the more complex the conditions, the more devious will be the rhythm, if for no other reason, because of the many factors that are set into play, often working at cross-purposes. Thus the regular breathing rhythm takes on a different form in certain pathological cases when the respiratory movements occur in groups separated by apnoeic pauses. As CHEYNE and STOKES described it, there is first an increase in the respiratory movements until the maximum is reached and then there is a falling off until they stop entirely, and EYSTER showed that there is a similar rhythmic wave of blood pressure under those conditions.[65]

Whatever sort of rhythmic phenomenon we might be studying, be it ever so complex, should be examined in the light of more elemental rhythms. The course here does not differ from the procedure in other and, sometimes, more complex processes. This attitude is instanced in SHERRINGTON's asking

"How there are developed from such minutely oscillatory nervous discharges those coarser rhythmic actions with periods recurring once in 3 secs., as in breathing, or 1 in a sec., as in the step, or 4 times a sec., as in the scratch reflex?"[66]

The tentative answer that SHERRINGTON gives to the question he has raised bears closely on what has been said above about the complexity of conditions in relation to the resulting rhythm.

"The suggestion has often been made," SHERRINGTON says further, "that such rhythmic nervous actions are the result of the concurrent action of two opposed nervous forces, the outcome of a constant opposition or resistance acting against a constantly discharging nervous activity."

And it may be added that there is nothing in the whole article to indicate that he finds fault with this suggestion. Applying this tentative explanation to the work described here, we may say that while rhythmic phenomena are to be expected almost in any task,[67] the *particular kinds of rhythmic phenomena* disclosed i[illegible]

[65] Howell: Text Book of Physiology, 2nd ed., p. 654.

[66] Sherrington: Nervous Rhythm Arising from Rivalry of Antagon[illegible] Reflexes. Proceedings Royal Society, London, 1913, vol. 86, Series B, p. 235

[67] *Cf.* the periodic fluctuation tendency in Berger's tapping experiments which were mentioned in the [illegible] literatu[illegible] *vide su*[illegible]

our records are due to the two opposing series of impulses, in other words, due to interference. We may even go further and hold that wherever we find rhythmic processes that suggest those mentioned earlier in this study, there is a likelihood that the result is due to interference conditions. Thus in SCHUYLER's work on the acquisition of skill in typewriting, a reference to rhythm is contained in the statement that

"In the curve of errors that which attracts attention is the remarkable persistence and regularity of the mistakes."[68]

Book also talks of "regular and irregular fluctuations"[69] explaining the former on the basis of the ANTRIEB and ANREGUNG or increase and relaxation of attention. In both of these investigations, the interference processes may have had much to do with the occurrence of periodic fluctuations. We shall have occasion to touch upon this subject again in discussing the results of the writing experiments.

THE GROUPING OF MISTAKES.

Marked off from the foregoing, though at first bush seemingly connected with it, is the tendency to continue going wrong once a start is made in that direction. One mistake would lead to utter confusion for a series of stimuli, until the subject, either by pausing or otherwise managing to gain control over the situation, is again able to proceed smoothly with his work. It should be made clear that "mistakes" is not used in a sense that would include omissions. The attitude involved in making a mistake seems to be a different one from that of omission; and if we were to adopt a term employed by ACH[70] we should be inclined to call the former a "*nichtintentionale Fehlreaktion,*" and the latter an "*intentionale Fehlreaktion,*" the degree of the difference varying with the different subjects.

This tendency of lapsing into a series of errors has rarely been alluded to in the literature, as YOAKUM observes,[71] but the oc-

[68] E. J. Swift and W. Schuyler: *Psych. Bulletin,* vol. IV, 1907, p. 310.

[69] Book: The Psychology of Skill, p. 120 and 123-127.

[70] Ach: Ueber den Willensakt und das Temperament, p. 270,*cf.* also p. 243 and 259.

[71] Yoakum: An Experimental Study of Fatigue. *Psychological Review, Monograph Supplements* 26, p. 65.

currence seems to be very common. Perhaps its very naturalness is the cause of its not receiving more publicity at the hands of investigators. Certainly it is easier to explain the clustering of mistakes than to find the cause of the first mistake made. In popular lore, there is many a saying among a number of peoples to the effect that one mistake (Sin, crime, etc.) brings on another. If we stop to analyse some of the ethical cases in which this maxim is true, we shall probably find that the situation, though of vastly greater consequence, is similar to that where the subject is required to perform a difficult experimental task.

In reporting the grouping of errors, YOAKUM states that he was

"Gratified to note that the same general phenomenon had been found in connection with some practice tests made on the typewriter by Mr. William Schuyler."[72]

It is hardly likely that a tendency of this sort would be so utterly ignored,[73] and even if it is mentioned rarely, it comes up in the literature sometimes in a somewhat different connection. BOOK, for instance, says

"A third fact revealed by our data was, that when the fluctuations in attention and effort above described were noticed by the learner, they were almost invariably followed by lapses in the efficiency of the effort put forth."[74]

What happens is just this: in a critical moment a wrong reaction is made. The subject notices the mistake, and this puts him out of gear. The mind becomes more and more befogged until a sufficient period has elapsed for recovery.[75] This half-dazed

[72] Yoakum: *loc. cit.*

[73] ACH does not report the occurrence of errors in groups, but we must remember that his conditions would not be favorable for such a phenomenon to come to light, as the task with his subjects was not a continuous one but discrete, so that there is really time between the single experiments for the subjects to pick up through sheer determination, even when as a result of several failures there developed a "Zustand der Verzweiflung mit einer Entsagung auf weiteres aktives Eingreifen" (Ach, *loc. cit.*, p. 273)

[74] Book: Psychology of Skill, p. 132.

[75] Mr. O. V. FRY who has been working on railway tests in the Harvard Psychological Laboratory noticed the same tendency on the part of the subjects in manipulating the throttle (results unpublished). He furthermo

state which is described in the introspection will often give rise to a whole series of random or blind reactions.

RANDOM REACTIONS.

The reactions given by subjects during the state of utter confusion are called random reactions. In reality they are not responses at all. The subject taps the keys without reference to the stimuli, but simply alternates first one movement for a time and then another, continuing so until he is able to make a fresh start on some stimulus he has fixed his attention upon. This is one way, in fact the only way, that those particular subjects who lapse into a state of confusion have of carrying out the instructions. This random tapping is not merely a reversal, i.e., where the movement and the stimulus have become wrongly associated, and the subject is reacting to the red stimulus with the movement representing the green stimulus, and *vice versa*. An examination of the records shows no correspondence whatever between the reactions and the stimuli. A single reversal, however, may have brought about the whole lapse.

In closing the chapter on the general results, it may be stated that these hold for all the series of simple movement experiments. We have, however, before us a body of data that bear on inter-serial and intra-serial relations. We may ask ourselves, for instance, how the results of one variation compare with those of another or whether the degree of interference in one revolution is more marked than in another. These and other questions of this sort which may suggest themselves are treated below under the caption of specific results.

declares that this situation is responsible for many a disaster, for frequently an engineer will run past several signals without taking the necessary precautions that the signals were intended to convey.

CHAPTER VI

Specific Results.

In the last chapter we have become acquainted with a number of facts in connection with interference, chief among which stands out the result that the easier of two movements is apt to be emphasized under pressure. From the two tables giving the results for the vertical and lateral movements, we have seen that there was a strong tendency to favor the vertical (downward) movements in every way. Not only were there fewer omissions here, but more additions, i.e., superfluous movements.

Let us now examine the results of the other series, all of which, as has already been intimated, indicate the same general tendency.

In the second series, it will be remembered, two fingers were employed to execute the two different movements, viz., the forefinger and the medius (of the left hand, as in the first series). Both movements were now downward taps on the keys. Much time need not be lost on finding out which of the two movements is the easier, since from common experience we know the middle finger to be both the slower and the more difficult of the two to manipulate separately. The explanation is not far to seek. The close relation between the two fingers affects the medius more than the forefinger which, owing to practice in the separate use of its muscles, has undergone some modification to the extent that it is not as inseparably bound up with the grasping reflex as are the other three fingers. In other words, the forefinger, being the less dependent of the two, in its relation with the middle finger, offers less inhibition when moved alone,[76] and is, therefore, other things equal, the easier finger to make a movement with. In the 30 secs. tests that were given for the sake of comparing the technical difficulty of the two different move

[76] *Cf.* H. S. Langfeld: Facilitation and Inhibition of Motor Impulses, *Psych. Review*, vol. XXII, 1915, p. 456.

ments, the larger number of reactions with the forefinger as compared with those where the medius was used proved that one is safe in assuming that the forefinger reaction would be technically easier than the medius reaction. This should be borne in mind in examining the next two tables.

TABLE III

Totals for All Subjects Taken Collectively. (Series II.)

Type of Movement	OMISSIONS		ADDITIONS		No. of requisite reactions	No. of stimuli
	Double	Single	Double	Single		
Forefinger	11.7%	4.6%	1.8 %	3.6+%	49,800	24,900
Middle finger	16.8%	6. %	.75%	2.3%	56,352	28,176

TABLE IV.

Results for Individual Subjects in Per Cent. (Series II.)

Subject	Type of Movement	OMISSIONS		ADDITIONS		30 secs. test
		Double	Single	Double	Single	
LV	Forefinger					
	Forefinger	1.75	1.65	20.86	28.7	$68\,^{8}/_{2}$ a
	Mid. finger	4.28	2.62	6.45	22.09	$66\,^{2}/_{2}$
RO	Forefinger	5.58	2.83	.31	8.25	83
	Mid. finger	8.42	5.26	.14	.32	$75\,^{2}/_{2}$
RU	Forefinger	7.3	2.3	.42	1.22	95
	Mid. finger	11.4	4.	.47	.64	$91\,^{1}/_{2}$
FG	Forefinger	4.37	5.82	.41	3.92	78
	Mid. finger	6.16	11.26	.42	1.77	$69\,^{1}/_{2}$
GD	Forefinger	9.	2.3	3.	5.1	unavailable
	Mid. finger	13.	3.45	.04	3.6	
LE	Forefinger	6.1	3.35	2.28	4.11	58
	Mid. finger	13.	5.77	1.07	2.42	$66\,^{1}/_{2}$ b
LO	Forefinger	8.53	5.	1.37	2.64	$72\,^{1}/_{2}$
	Mid. finger	13.13	5.8	.74	1.3	67
LL	Forefinger	10.	10.81	.3	2.3	$64\,^{2}/_{2}$
	Mid. finger	16.58	9.77	.13	1.04	$48\,^{10}/_{2}$
TT	Forefinger	10.34	12.25	1.40	6.72	unavailable
	Mid. finger	18.9	16.	1.32	2.84	
ML	Forefinger	15.5	7.86	.13	.57	$67\,^{1}/_{2}$
	Mid. finger	21.9	6.71	.12	.25	63
SR	Forefinger	24.9	1.41	.24	.55	$78\,^{8}/_{2}$ c
	Mid. finger	29.6	2.56	.18	.3	$71\,^{1}/_{2}$
LZ	Forefinger	29.	2.8	.05	.25	$60\,^{6}/_{2}$
	Mid. finger	38.	3.84	none	none	64
			Average			
	Forefinger	11.03	4.86	2.56	4.74	72.5
	Mid. finger	16.19	5.58	.92	3.04	70.

In table III we note again that the easier movement is favored both by the smaller number of omissions and the greater number of additions. This tendency is consistently exhibited throughout the second series, sporadic cases of emphasis on the more difficult movement having already been explained as due to extraneous factors (after-images with a particular color, for instance) and amply accounted for in the introspection of the subjects.

That the results of the individual subjects present the same general phenomenon will readily be seen from the preceding table.

Remarks on the 30 secs test. (Table IV)

(a) The instructions were to tap in pairs just as had been done in the regular experiments; hence, the single taps were counted separately.

(b) The writer has not been able to explain this seeming anomaly on the part of LE. The computation of the test record was made long after there was any opportunity of investigating the discrepancy.

(c) There is really no comparison here, because subject SR tapped in pairs only with the forefinger. The tapping with the middle finger was done continuously, hence the *Aufgabe* was not kept the same for both. There was a great deal more deliberateness with the forefinger tapping as shown by the regularity of the pairs.

Comparison of Series I and II.

Judging from the total percentage of both omissions and additions in the two series respectively, one would be inclined to say off-hand that inhibition (and confusion) is more marked in the two finger (forefinger and middle finger) combination than in the vertical-lateral (forefinger alone) arrangement. In all probability that is so, though the fact could not be actually demonstrated, seeing that several subjects dropped out just after the beginning of the second series, and were replaced by others. The new subjects, LZ and LV, must have affected the average materially, though in opposite directions For this reason, however,

it might be said that the one offset the other. RO and RU show a great increase in omissions. In part this is to be explained by the fact that the period had been shortened for them from 33 secs. to 28 secs. Yet the slight increase of rate alone could not have been responsible for the four-fold increase of double omissions in the case of RO and the six-fold increase in the case of RU. Besides, several other subjects give evidence of a similar increase, in spite of the practice that they have had. The two slowest subjects, TT and SR, however, on the contrary, show a marked improvement, which is greater for the more difficult movement, though the easier is still the one to be emphasized, but the gap now is not so great as it was in the first series.

Granted that there is, all things considered, more inhibition with the two movements of the second series, we must guard against assuming that the forefinger reaction of the latter is the same as that in the former series. As far as the instructions are concerned, they were all the same, and the subjects might have thought that, as in the first series, they were performing a vertical downward movement with the forefinger, which is true enough, but we must not lose sight of the circumstance that while in the first series the forefinger was the only finger outstretched, the rest of the hand being closed, the subjects had, in the present series, two neighboring fingers outstretched, *mutually inhibiting each other all the while.* This is an important point to take cognizance of; for it emphasizes the need of studying two given movements not only in relation to, but also as *actual functions* of each other. Before we can ascertain the relation, it *is necessary to study the one in the light of the other.* When one finger performs two different movements, the dependence is not so great as when two different (and adjacent) fingers perform each a similar movement. Hence it would not be safe to conclude that because the second series presents a greater degree of inhibition, and the vertical movement is common to both series, therefore the downward movement for the medius is responsible for the added difficulty and is more susceptible to interference than the lateral movement of the fore-

inger. What mars the validity of this argument is the assump-ion that the downward forefinger movement is common to both ;eries. In reality, this movement is more hampered in the second ;eries, and this seems to be a contributing factor in the increase)f inhibition.

In passing to the results of the third series, we must again ake this reciprocal inhibitory effect of the two fingers into con-;ideration before we can arrive at any conclusion as regards the nhibitive coefficient of each finger separately.

Series III. (Middle finger and ring finger)

The third series offers the most irregular results, irregular in he sense that it does not bear out our expectations. In the irst place, we should suppose that the set of movements involv-ng the middle finger and the ring finger would be more difficult o carry out than those performed by the forefinger and the niddle finger. Our results show there is less inhibition here, and)nly slightly more than in the first series, as will be seen from he following table in comparison with tables I and II.

TABLE V.

Totals for All Subjects. (Series III.)

Type of Movement	OMISSIONS		ADDITIONS		No. of requisite reactions	No. of stimuli
	Double	Single	Double	Single		
Middle finger	8.76%	5.004%	.723%	2.03 %	35,688	17,844
Ring finger	14.674%	7.655%	.294%	.942%	40,152	20,076

The deviation from what might have been expected is still nore noticeable when we come to examine the individual per-:entages. What stands out particularly is the curious shift of)laces in the efficiency order of several of the subjects, showing learly that there are vast individual differences in connection vith the movement of the two different fingers. There is no loubt that improvement in genera)lays a great par' 1er^ This

can be seen by comparing the 30 secs. tests in the second and third series (vide tables IV-VI). Meanwhile let us glance for a moment at the individual percentages of omissions and additons in the third series.

TABLE VI.

Results for Individual Subjects in Per Cent. (Series III.)

Subject	Type of Movement	OMISSIONS		ADDITIONS		30 secs. test
	Downward	Double	Single	Double	Single	
LV	Mid. finger	1.59	2.1	1.75	6.12	81 4/2
	Ring finger	2.81	4.46	.69	2.85	73 8/2
RO*	Mid. finger	3.75	3.96	.46	.36	83 1/2
	Ring finger	4.88	3.91	.046	.55	77
RU*	Mid. finger	8.89	1.23	.25	.72	94
	Ring finger	14.54	2.29	.13	.5	88 1/2
LO	Mid. finger	4.84	3.56	3.41	3.56	78 1/2
	Ring finger	13.48	3.68	1.46	1.52	67 1/2
FG	Mid. finger	4.99	10.39	.823	2.52	80 4/2
	Ring finger	8.51	12.984	.414	1.24	84
LL	Mid. finger	5.19	11.72	.31	2.21	66
	Ring finger	11.51	13.58	.092	1.19	57 1/2
LE	Mid. finger	11.25	3.846	.64	2.63	67
	Ring finger	19.73	11.15	.12	1.28	61 1/2
LZ	Mid. finger	13.52	2.42	.1	1.8	73 1/2
	Ring finger	22.375	7.91	.046	.5	72
ML	Mid. finger	11.67	7.45	none	.256	66
	Ring finger	22.34	11.41	none	.092	62
SR	Mid. finger	26.05	1.76	none	.21	78 1/2
	Ring finger	31.35	4.13	.18	.06	67 1/2
			Average			
	Mid. finger	9.17	4.84	.77	2.03	77.8
	Ring finger	15.15	7.45	.31	.97	73.2

In this series subjects LO and LZ give evidence of most marked improvement, while subjects LE and ML, on the other hand, present a drop in the work. The other changes are slight. It will also be seen that LV now makes far fewer additional taps than in the former series.

Series IV.

The most difficult set of movements was by far that of the fourth series in which the subjects made an upward movement

* It will be remembered that subjects RO and RU were given 28 secs. per revolution, while all the rest had 33 secs. per revolution.

with the nail part of the forefinger (dorsal or posterior) and a downward movement in the usual way, i.e., with the tip of the same finger (volar or anterior). These two types of movement have also been referred to previously as extensor and flexor reactions respectively.

The upward movement after a very short period became painfully fatiguing. Hence the high percentage of omissions in this series as shown by the following table.

TABLE VII.

Totals for All Subjects. (Series IV).

Type of Movement	OMISSIONS		ADDITIONS		No. of requisite reactions	No. of stimuli
	Double	Single	Double	Single		
Forefinger						
Flexor	12.345%	8.7%	.682%	1.34%	27,168	13,584
Extensor	21.98 %	8.95%	.276%	1. %	34,728	17,364

In the individual results we note another change in order. Subject FG is not only the least affected by the introduction of the upward movement, but is the only one for whom the present series seemed to be markedly easier than the previous series. A slight gain is also shown by ML. With the exception of FG, the quick reagents proved to be relatively much more affected by the increased difficulty than the slow reagents (cf. RC, RU and LV). There is also with them a relatively greater divergence between the two movements.

The most striking anomaly in these results is presented by LL who, both in the regular experiments and the 30 secs. test, slightly favors the extensor movement. His playing the violin does not seem to have any connection with this result, as there were other instrumentalists among the subjects, but none besides LL gave evidence of this remarkable deviation.

The relation between flexor and extensor movements had already been taken up in several investigations, and it is generally agreed that the former of the two movements is the more

TABLE VIII.

Results for Individual Subjects in Per Cent. (Series IV.)

Subject	Type of Movement Forefinger	OMISSIONS		ADDITIONS		30 secs. test
		Double	Single	Double	Single	
FG	Flexor	2.94	11.55	.56	1.75	80 1/2
	Extensor	6.98	14.3	.56	3.04	70
LV	Flexor	5.04	6.03	1.97	4.93	88
	Extensor	11.71	9.35	.709	4.	65
RU	Flexor	17.86	3.78	.14	.91	90 1/2
	Extensor	22.18	3.66	.45	1.07	74
RO	Flexor	15.12	5.81	.21	.21	84 1/2
	Extensor	21.56	11.93	none	.56	67 1/2
LO	Flexor	6.58	4.9	3.64	4.06	68
	Extensor	23.93	7.48	1.12	1.18	65 1/2
ML	Flexor	10.38	4.88	none	.72	75 1/2
	Extensor	24.26	6.62	.04	.16	62 1/2
LE	Flexor	13.65	13.65	.35	1.19	78
	Extensor	20.6	11.31	none	.22	56
LL	Flexor	19.88	23.66	none	.21	50 1/2
	Extensor	22.97	17.45	none	.337	53 1/2
SR	Flexor	29.22	5.82	none	.11	86 1/2
	Extensor	31.33	4.08	none	.08	64
LZ	Flexor	21.14	4.55	.14	.28	unavailable
	Extensor	33.55	27.	none	.61	
			Average			
	Flexor	14.18	8.46	.7	1.43	78.44
	Extensor	21.9	11.31	.28	1.12	64.34

favored, and, more favored because the easier and the more rapid to perform.

CAMERER,[77] who seems to have been the first to compare these two movements experimentally, was led to the conclusion that extension is easier than flexion, and BAUCH[78] is at a loss to square this conclusion with his own results which indicate a perfect correlation between the more favored (*bevorzugt*) and the easier (*bequemer*) movement. In fairness to CAMERER it ought to be said that BAUCH overlooks the conditions of the original experiment. If CAMERER had compared flexor and extensor movements in a vertical plane, and, what is even more important, of

[77] W. Camerer: Über den Zeitlichen Verlauf d. Willensbewegungen, Zt. für Biologie, vol. XLVII, N. S. vol. XXIX, 1906, p. 272.

[78] M. Bauch: Zur Gleichförmigkeit der Willenshandlungen, Fortschritte der Psych. etc. vol. II, 1914, p. 366.

the fingers or wrist, (forearm), there can be no doubt that the results would tally with those of other investigators, including BAUCH. The movements, however, with which CAMERER was concerned were in the horizontal plane, as is plain from his description in his article.[79] In view of this, the characteristic feature of the opposition is not that between flexion and extension, but rather between centripetality and centrifugality; and even the latter distinction would have to be further qualified, as ordinarily the centripetal movement is affirmed to be easier than that away from the body (centrifugal).

All this goes to prove how important it is to specify, in every case, if not the anatomical mechanism involved in a given movement, at least the position of the limb performing the movement as well as the plane and the direction in which the movement is made. Flexion and extension of the arm involving the biceps and triceps muscles are certainly not in the same class with the flexion and extension of the hand or the individual fingers where the ulnar flexor and extensor muscles as well as their tendons are involved. For this reason, it would not be safe to apply a conclusion drawn for a given set of flexor and extensor movements to all such antagonistic sets we can think of; and while possibly CAMERER's designation of the antagonism was a misnomer, BAUCH was not warranted in supposing that the former's results would make the easier movement the less favored.[80]

Besides BAUCH, who has already been mentioned as identifying flexion with the easier movement and extension with the more difficult, GAULT[81] has found that flexion is the more rapid

[79] *loc. cit.*

[80] Bauch's mode of reasoning, when stated in succint form, is this: Several investigators, including myself, have found flexions more favored than extensions; (2) but Camerer has found flexions more difficult (unbequem) than extensions to perform; hence, (3) the more difficult movement is the more favored movement. If we took the trouble to arrange this bit of inference, formally, we should probably be able to detect in these three propositions the fallacy of undistributed middle, or perhaps rather ambiguous middle, giving us four terms instead of three. Bauch's reference is to Camerer's dissertation of 1866, and not the article of half a century later.

[81] R. H. Gault: On Conditions Affecting the Minimal Rate of Voluntary Extensor and Flexor Movements of the Right Arm. *American Journal of Psychology*, pp. 370, 383.

of the two. Here, too, the movements seem to be in the horizontal plane, but the upper arm is not used to the same extent as in CAMERER's experiments.

There seems, at first sight, to be a wide divergence in the percentage of the omissions for the two movements in the fourth series, and that is really what we should have expected considering that the grasping reflex and the natural semi-flexed position of the fingers would tend to strengthen the flexor mechanism, while, on the other hand; the fingers are comparatively seldom held in a rigidly outstretched position. Yet if we examine the results of the various series, we shall find that both in the percentile totals and percentile averages of omissions and additions, taken together, there is greater approximation between the two movements of the fourth series than for any of the other series.

How can we account for this fact? Only one form of explanation suggests itself to us and that is to assume that the high degree of mutual opposition between the flexor and extensor movements has been reduced considerably as a result of practice. The improvement, accruing from practice would consequently not only affect the motor ability of the fingers, and reduce the actual number of omissions (compare second and third series), but would also tend to overcome the strength of the dominant determining tendency and reënforce the determining tendencies touched off by the instructions. In other words, the easier movement would not, relatively speaking, be emphasized so much as originally. A tendency towards such approximation would indicate improvement in the direction of greater resistance.

In order to facilitate comparison of the various results, a table is here appended giving the totals and averages for all the four series.

From the preceding table it is clear that there is a general tendency for an increment in the difficulty of the one movement to affect the other more or less proportionally; in other words, *increase the difficulty of either of two movements,* and the result is that *both movements are made more* difficult, i.e., a redistribution of inhibition values will take place.

TABLE IX.
Comparison of All the Series.

Type of Movement	OMISSIONS Double	OMISSIONS Single	ADDITIONS Double	ADDITIONS Single	Inhibition Ratio[82]
Forefinger		SERIES I. *Totals for All Subjects.*			
Vertical	8.8%	5.%	.55%	1.65%	
Lateral	12.8%	7.06%	.37%	.92%	58.7%
		Average			
Vertical	8.8%	5.73%	.56%	1.9%	
Lateral	13.9%	8.21%	.347%	.903%	59.15%
Downward		SERIES II. *Totals for All Subjects.*			
Forefinger	11.7%	4.6%	1.8%	3.6%	
Mid. finger	16.8%	6.%	.75%	2.3%	59.1%
		Average			
Forefinger	11.03%	4.86%	2.56%	4.74%	
Mid. finger	16.19%	5.58%	.92%	3.04%	51.57%
Downward		SERIES III. *Totals for All Subjects.*			
Mid. finger	8.76%	5.%	.723%	2.03%	
Ring finger	14.67%	7.65%	.294%	.94%	52.9%
		Average			
Mid. finger	9.17%	4.84%	.77%	2.03%	
Ring finger	15.15%	7.45%	.31%	.97%	54.23%
Forefinger		SERIES IV. *Totals for All Subjects.*			
Flexor	12.34%	8.7%	.66%	1.34%	
Extensor	21.98%	8.95%	.27%	1.%	59.81%
		Average			
Flexor	14.18%	8.46%	.7%	1.43%	
Extensor	21.9%	11.31%	.28%	1.12%	63.61%

[82] The inhibition ratio was obtained by dividing the results of the more difficult movement into those of the easier after making due allowance for the additions and giving half values to the single reactions of both the omissions and additions. The formula may be stated thus

$$\frac{D.O. + \frac{S.O.}{2} - D.A. - \frac{S.A.}{2} \quad \text{(easy)}}{D.O. + \frac{S.O.}{2} - D.A. - \frac{S.A.}{2} \quad \text{(difficult)}}$$

D.O. = double omissions
S.O. = single omissions
D.A. = double additions
S.A. = single additions

Converting the omissions to positive values and adding thereto the additions would of course yield a much higher ratio, as this would be computed in terms of *achievement*· but the order of the ratios is not affected in either

From the inhibition ratio values, we may infer that the greater the practice the more chance is there for individual variation. For in the first series there is scarcely any difference between the percentile total and percentile average. The discrepancy between the two in the second series is due, in all probability, to the fact that several new subjects had come into the experiment during this series. We should say that this circumstance brought about a greater variation from the mean. The difference is greatly reduced in the third series, but yet, as the table shows, greater than for the first series, while in the fourth series, there is another slight rise. The effect of unequal practice, then, is the only thing we can think of to account for this regular phenomenon.

TABLE X.

Showing Facility of Different Finger Movements.

	Series I		Series II		Series III		Series IV	
	Forefinger		vertical		downward		Forefinger	
Double reactions per 30 seconds	Vertical downward	Lateral centripetal	f. f.	m. f.	m. f.	r. f.	flexor	extens
	Tests not given		72.5	70	77.8	73.2	78.44	64.34

Just what the facility ratios are of the different movements to one another cannot be told from the data in hand, because we don't know how much allowance to make for practice, but we can easily present the different movements in the order of their facility (i.e., objective ease) from the last two tables. In this scale we can also find a place for the lateral movement (though there is no separate test for it) by comparing first the inhibition results of the different series, and then the individual finger tests. The order accordingly would be as follows:

left hand:
1. Forefinger vertical downward—the easiest
2. " lateral centripetal—next easiest
3. Middle finger vertical downward—more difficult
4. Ring finger " " —more difficult
5. Forefinger extensor—most difficult.

way. And as the differences (curve) show better with the negative values as our standard, we have chosen the former method.

Interference Curve.

We now have the question before us of the efficiency curve during interference. We can hardly expect the subjects to do as well in the last revolution as in the first revolution. This we know: there will be a decline in the achievement, but what course will that process take? Will it follow the *treppe* effect of the simplest muscular fatigue? Does it exhibit the drop in the form of a straight line, or does it reveal the influence of the *Antrieb, Anregung, Gewoehnung,* and other such Kraepelinian factors?

On this point we cannot look for any clear-cut results in our investigation for several reasons. In the first place, the different series, as will be seen presently, offer too much variation, as regards this particular matter, for a decisive reply. Then there is also the special difficulty of applying explanations that can be employed with such great latitude and indefiniteness. This is true not only of Kraepelin's proposed factors to account for the fluctuations in the course of fatigue, but of all others as well; and Thorndike, in his ruthless criticism of Kraepelin's findings has scarcely succeeded in fortifying his own position. We don't know any more about the "satisfyingness" and "annoyingness" of a given task than we do about the warming-up process or initial and end-spurts, perhaps even less, for the latter are comparatively simple processes, while "satisfyingness" comprises a number of contributory factors.[88]

The charge of *ex post facto* explanation which Thorndike lays at the door of Kraepelin's laboratory holds equally well of his own view, since so far, we have not been able to learn the *modus operandi* of the different factors invoked, and consequently we are unable to tell whether the one factor was operative or another, at a given point. In fact, any or all of the enumerated factors might have been responsible for a dent in the curve.

The interference curve, because of the four different components it embraces, is presented here in tabular form for the sake of convenience.

[88] E. L. Thorndike: Educational Psychology, vol. III, p. 78.

TABLE XI.

Series I.

	Vertical	Lateral	Combined
Revolution 1.	$-97\frac{90}{2}+14\frac{45}{2}$	$-176\frac{147}{2}+5\frac{26}{2}$	$-273\frac{137}{2}+19\frac{71}{2}$
" 2.	$-133\frac{101}{2}+16\frac{51}{2}$	$-210\frac{158}{2}+18\frac{27}{2}$	$-343\frac{258}{2}+34\frac{78}{2}$
" 3.	$-140\frac{113}{2}+12\frac{49}{2}$	$-247\frac{192}{2}+13\frac{30}{2}$	$-387\frac{305}{2}+25\frac{79}{2}$
" 4.	$-163\frac{101}{2}+24\frac{55}{2}$	$-278\frac{165}{2}+14\frac{32}{2}$	$-441\frac{266}{2}+38\frac{87}{2}$
" 5.	$-184\frac{136}{2}+18\frac{72}{2}$	$-276\frac{213}{2}+14\frac{26}{2}$	$-460\frac{349}{2}+32\frac{98}{2}$
" 6.	$-194\frac{133}{2}+14\frac{37}{2}$	$-279\frac{196}{2}+6\frac{40}{2}$	$-473\frac{329}{2}+20\frac{77}{2}$

EXPLANATIONS.

The minus sign signifies omissions, the plus sign, additions. The whole numbers stand for double reactions; the halves indicate single reactions.

Each revolution represents 33 secs.

The omissions and additions in each revolution are out of a total number of 2425 double reactions per revolution for the vertical movement and 2512 double reactions per revolution in the lateral movement.

In the above table we find a pretty regular drop, more plainly seen in the case of the easier movement. There seems to be no room in this series for the *Antrieb, Anregung,* or any of the other agencies believed to operate in continued work.

In the next series, however, we get quite a meandering of the line, and it might be possible to regard the fluctuations as evidence of change of attitude on the part of a number of the subjects. Let us see how this series compares with the last.

The sudden drop in the efficiency curve (next table) between the first revolution and the second, as well as the rise between the fourth and fifth, and also the fact that there is no further drop in the sixth revolution, would tend to confirm the findings of KRAEPELIN and to argue for the identity of the interference curve and the fatigue curve, which would only mean that both processes are governed by common factors.

Table XII.

Series II.

	Forefinger	Middle finger	Combined
Revolution 1.	$-337\frac{194}{2}+62\frac{153}{2}$	$-500\frac{278}{2}+29\frac{121}{2}$	$-837\frac{472}{2}+91\frac{274}{2}$
" 2.	$-464\frac{180}{2}+66\frac{136}{2}$	$-715\frac{286}{2}+31\frac{104}{2}$	$-1179\frac{466}{2}+97\frac{240}{2}$
" 3.	$-458\frac{207}{2}+68\frac{138}{2}$	$-837\frac{259}{2}+32\frac{98}{2}$	$-1295\frac{466}{2}+100\frac{236}{2}$
" 4.	$-555\frac{187}{2}+68\frac{138}{2}$	$-918\frac{294}{2}+36\frac{110}{2}$	$-1473\frac{481}{2}+104\frac{248}{2}$
" 5.	$-564\frac{196}{2}+92\frac{183}{2}$	$-888\frac{291}{2}+43\frac{117}{2}$	$-1452\frac{487}{2}+135\frac{300}{2}$
" 6.	$-558\frac{209}{2}+91\frac{173}{2}$	$-867\frac{336}{2}+43\frac{127}{2}$	$-1025\frac{545}{2}+134\frac{300}{2}$
	4150 double reactions per revolution	4696 double reactions per rev.	

Before making, however, any further conjectures in that direction let us glance at the curves of the other two series.

Table XIII.

Series III.

	Middle finger	Ring finger	Combined
Revolution 1.	$-179\frac{130}{2}+20\frac{60}{2}$	$-347\frac{283}{2}+14\frac{27}{2}$	$-526\frac{413}{2}+34\frac{87}{2}$
" 2.	$-241\frac{152}{2}+15\frac{52}{2}$	$-457\frac{265}{2}+4\frac{37}{2}$	$-698\frac{417}{2}+19\frac{89}{2}$
" 3.	$-246\frac{162}{2}+20\frac{44}{2}$	$-479\frac{251}{2}+8\frac{42}{2}$	$-725\frac{413}{2}+28\frac{86}{2}$
" 4.	$-299\frac{169}{2}+22\frac{58}{2}$	$-545\frac{259}{2}+14\frac{28}{2}$	$-844\frac{428}{2}+36\frac{86}{2}$
" 5.	$-308\frac{130}{2}+19\frac{70}{2}$	$-556\frac{240}{2}+9\frac{29}{2}$	$-864\frac{370}{2}+28\frac{99}{2}$
" 6.	$-298\frac{140}{2}+33\frac{74}{2}$	$-565\frac{226}{2}+12\frac{32}{2}$	$-863\frac{366}{2}+45\frac{106}{2}$
	2974 double reactions per revolution	3346 double reactions per rev.	

Upon examination it will be noticed that the curve in the third series bears a close resemblance to that of the second series. There will be found here the same wide gap between the first and second revolutions, and the same approximation between the fifth and sixth revolutions, this time in favor of the latter, however.

The same general results are obtained for the fourth series, even in a more pronounced form.

TABLE XIV.

Series IV.

	Flexor forefinger	Extensor f.f.	Combined
Revolution 1.	$-252\frac{182}{2}+8\frac{29}{2}$	$-470\frac{246}{2}+7\frac{37}{2}$	$-722\frac{428}{2}+15\frac{66}{2}$
" 2.	$-315\frac{202}{2}+12\frac{22}{2}$	$-613\frac{237}{2}+14\frac{37}{2}$	$-928\frac{439}{2}+26\frac{59}{2}$
" 3.	$-328\frac{211}{2}+16\frac{33}{2}$	$-656\frac{275}{2}+8\frac{39}{2}$	$-968\frac{486}{2}+24\frac{72}{2}$
" 4.	$-349\frac{206}{2}+18\frac{29}{2}$	$-687\frac{285}{2}+6\frac{29}{2}$	$-1036\frac{491}{2}+24\frac{58}{2}$
" 5.	$-316\frac{209}{2}+15\frac{39}{2}$	$-676\frac{268}{2}+11\frac{24}{2}$	$-992\frac{477}{2}+26\frac{63}{2}$
" 6.	$-328\frac{175}{2}+21\frac{35}{2}$	$-704\frac{268}{2}+2\frac{18}{2}$	$-1032\frac{443}{2}+23\frac{53}{2}$
	2264 double reactions per rev.	2894 double reactions per rev.	

Again we notice the great drop in the second revolution followed by a gradual decline in the second and third revolutions and then a distinct gain in the fifth revolution, with the usual approximation between the fifth and sixth revolutions, which means that the worst work was done not in the last revolution, but in the fourth.

Our tabular curves, we can say then, exhibit the signs of a certain regularity in the course of the work—regularity in the sense of uniformity of phenomena. The first series shows greater regularity in the direction of a straight line and less conformity to the curves of the other series.

It is safe, on the basis of our curves, to declare that there were regular fluctuations in the efficiency of the subjects during the work, but what these fluctuations are due to can at most only be conjectured. So many factors have been proposed that any one of them or several combined might be called in to solve the problem. Such a solution, however, is far from being satisfactory, for the reason that it is too easily manipulated. There is also the probability that under interference conditions, other factors are brought into play which operate only slightly or not at all in work of an ordinary kind. The solution of such problems must of necessity be reserved for future investigations in which the factors supposed to be responsible for drops or gains in efficiency can be tested separately.

Spontaneous Reaction.

The spontaneous tests which were given for the purpose of discovering differences between work under normal conditions and work under pressure brought out the following facts: (1) A good deal of automatism is displayed in the records; (2) the difference between the flexor and the extensor results is more marked for all but one subject here than when working under outside pressure in the form of rapidly alternating stimuli; (3) metabolic fatigue plays a secondary part in the waning of efficiency as disclosed by our interference curves; (4) The subjects fall into two classes according as they can accomplish more (a) while reacting to the rapidly alternating stimuli, or (b) tapping spontaneously with no stimuli in sight.

Before we proceed to give illustrations, let it be repeated here that in these spontaneous tests, the subjects were asked to tap the key in pairs just as they had done all along, and in order to eliminate automatism, they were given the further instruction to try and follow the general arrangement of the stimuli-lists they have been reacting to previously. This was a positive instruction with a negative significance. The purpose in view was not to have them lapse into a rhythmic repetition of the previous reaction, but to alternate at random between the flexor and the

extensor movements on the keys.[84] The working period lasted for 3 minutes, and each 30 sec. interval was marked off on the kymograph so as to represent the six revolutions in the ordinary experiments. In this way we could trace the achievement curve of the subjects for every 30 secs.

A transcription of some of the spontaneous records will serve to show what is meant here by automatism. In some cases, the tendency to mechanize is not displayed till the last 30 seconds of the test. The transcriptions of the kymograph records to follow are taken from subjects of different types. In addition, a sample will be shown of the subject least given to automatism, but it must also be stated that this subject, regardless of the fact that no stimuli-list contained groups of 5, 6 or 7 dots (and only one or two contained groups of 4) struck as many as 7 pairs in these tests. The greater scope for variation then may be taken as a partial explanation accounting for the absence of automatism in his case.

Tables XV and XVI, in which the mechanization lapses of the subjects are indicated by the numerals in bold type, are sufficient, we trust, to warrant the conclusion that rhythmic tendencies play a great part in work, and that each subject has his own favorite arrangement. Simple alternation of individual pairs seems to be more universal than other rhythmic arrangements.

As to the degree of physical fatigue that enters into a 3 minute working period, the tables stating the number of reactions for every 30 secs, point to the view that muscular fatigue does not play such a large part as might have been supposed. Of course, it may be urged that in the experiments with exposed stimuli, there is an expenditure of energy in the processes of perception and discrimination. All this is granted, but it will be recalled that in their introspection, some of the subjects spoke of being exhausted in the first minute of the experiment, and actually referred to muscular fatigue in the second revolution.

[84] The instructions really implied then that (a) there were to be no single reactions, (b) that a group was not to consist of more than three pairs, and (c) the immediately preceding group or individual pair might be repeated, but *only once*. Such were the conditions involved in the ordinary experiments.

Table XV.

Transcription of Spontaneous Records.

Subject FG Flexor movement.

1st 30 secs.	2 2½ 2½ 2½ 2 2 2 2 4 1½ 1½ 3 3 3
2nd " "	1 4 8 3 4 2 4 2 4 2 2 2
3rd " "	2 2 4 2 2 2 4 2 4 2 4 2 4 2
4th " "	4 5 2 2 2 2 4½ 2 2 3 2 6

Subject FG Extensor movement.

1st 30 secs.	1½ 2 2½ 1½ 2 1½ 1½ 2 2 4 1 2½ 3 4 3
2nd " "	4 1½ 7½ 3 4 2 4 2 4 2
3rd " "	2 4 2 4 2 2 2 4 2 4 2 4
4th " "	2 4 4½ 2 4 1½ 2 3½ 2 3 2 4

Subject LO Flexor movement.

1st 30 secs.	1 2 2 4 1 5 1 1½ 2 6 1 1½ 2½ 1 1 1 1
2nd " "	2, 1½, ½ 3½ 1 1 1 1 4 1 1½ 2 1 3 1 1 2 1 1 1½ 1½
3rd " "	2 1 1 4, ½ 4½ 1 2 1 1 2 1 3 1 1 1 1 3
4th " "	1, 1, 1, 1, 1, 4, 1, 1, 4, 1, 3, 1 1 1 2 1 1 2½ 1½ 1 3
5th " "	1, 1, 5½, 1, 2, 1½, 2, 1, 1, 1, 1, 3, 5½ 1 2 1 1½, ½ 3 1 2
6th " "	1 1 1 1 1½ 1½ 2 4 1 1 1 1 1 1 4 1 1 1 4 5

Subject LO Extensor movement.

1st 30 secs.	1 3 1 5 3 4½ 4 2 3 3½ 1 4 2 3½
2nd " "	1 1 2 1 2 1½ 1 2 1 1 1 4½ 4 1 1 1 1 2 1½ 1 1
3rd " "	1 1 1½ 3 4 3 1 1½ 3 1 2 1 2 1 1 1½ 3
4th " "	1 1 2 1 2 2 3 4 2 2½ 1½ 1 1 1 1 4 1 1 2 1
5th " "	1 2 2½ 1½ ½ 3 1 2 3 1½ 2½ 1 1 1
6th " "	1½ 1 1 1 1 1 1 1 1½ 2 1 1 1 1 1 1 1 1 1 1 1 1 1½ 1 2

Subject SR

It is sufficient to give the values only for the 1st and last 30 sec. periods in this case

1st 30 secs.

flexor	3 4 2 1 1 1 1 1 4 4 2 2 8 2 1
extensor	2 7 1 1 1 1 1 1 4 4 2 2 4

6th 30 secs.

flexor	3 3 2 9 1 1 1 1 1 1 1½ 1½ 1½ 1 1 1 1½ 1 1 1 1 1 1 1½
extensor	1 3 3 5 1 1 1½ 1 1 1½ 1 1 1 1 1 1 1 1 1 1 1 1 1 1

Table XVI.

Record of RO, showing practically least automatism

1st 30 secs.

flexor	2 3 1 2 6 3 7 2 3 1 1 3 7 1
extensor	4 2 1 3 3 2 6 2 1½ 1 2 5 2

2nd 30 secs.

flexor	1 6 1 1 2 2 2 4 2 6 1 1 3 1 2½ 3 1 1 5
extensor	3 2 2 3 4 1 2 2 4 1 1 4 1 2½ 1½ 2 1 3 3

3rd 30 secs.

flexor	4 1 3 1 4 1 3 3 2½ 1½ 4 6 2 2 6
extensor	1 1 1 1 3 1½ 1½ 2 2½ 1½ 1 6 3 3 4 4

4th 30 secs.

flexor	6 2 4 2 1 1 3 1½ 2 1 2 2 7 2 1 1 4 2 1½ 1½
extensor	2 1 2 2½ 1½ 1 1 2½ 2 4 2 1½ 4 3 1 1 2

5th 30 secs.

flexor	6 2 3½ 2 2 1 7½ 5 5 1 2 2
extensor	1 3 2 2 4 1 2 4 3 1 1 3

In the efficiency curve of the experiments with exposed stimuli, far greater regularity was exhibited. No subjects did, for instance, as good work in the last revolution as in the first. A drop was noticeable throughout until the fourth revolution. It was only a question of how steep the slope was. The indentations and fluctuations of the curve were few and regular. In the spontaneous reactions, however, we get results like these:

TABLE XVII.

	Subj. FG	Subj. LO	Subj. LZ	Subject SR		Sub
	Extensor	Flexor	Extensor	Flexor	Extensor	Flex.
1st 30 secs.	$31\frac{6}{2}$	$33\frac{3}{2}$	$27\frac{12}{2}$	37	31	$43\frac{1}{2}$
2nd " "	$34\frac{2}{2}$	$30\frac{6}{2}$	$14\frac{25}{2}$	$37\frac{1}{2}$	$34\frac{1}{2}$	$60\frac{9}{2}$
3rd " "	34	$30\frac{2}{2}$	$27\frac{19}{2}$	26	38	$49\frac{6}{2}$
4th " "	$35\frac{3}{2}$	$33\frac{2}{2}$	$30\frac{11}{2}$	29	34	$50\frac{7}{2}$
5th " "	—	$36\frac{5}{2}$	$31\frac{9}{2}$	29	$34\frac{1}{2}$	$56\frac{9}{2}$
6th " "	—	$42\frac{2}{2}$	$29\frac{8}{2}$	$38\frac{6}{2}$	$32\frac{2}{2}$	$43\frac{6}{2}$

What this marked difference between the interference curve and the working curve for the same period of time is due to we are not in a position to say. The deviation, however, clearly suggests that different elements are involved in the two processes. The question that presents itself is not only what part fatigue plays in our interference conditions, but the converse, viz., how much of what generally goes under the name of fatigue is directly due to interference? Had we a definite criterion of fatigue under all conditions, our problem should not be a difficult one to solve, but it seems that all we know about the fatigue curve in the more complex cases of work is that it tends generally in a downward direction. With this as our guide, it is plain we cannot measure some other factor which

may be either closely or remotely related to fatigue. Nevertheless even with this negative information as a starting-point, an investigation in which both fatigue and interference were studied separately and the one exclusive of the other, would, in the opinion of the writer, go a long way towards answering the double question just put.

In comparing the 'spontaneous' and the 'forced' (rate) records as to achievement, there are certain obstacles that could not be obviated. The most careful calculations cannot give us an idea of what allowance to make for the various elements present in the one and absent in the other. Excluding these considerations, a gross comparison of the two sets of records leads to the conclusion that most of the subjects, and possibly all, made more reactions when the pace was set than when they were left to their own inclination in the 'spontaneous' tests. The subjects about whom there is considerable doubt are LV, SR, and LZ. They accomplished more in the 'spontaneous' work, but it must not be forgotten that the time lost in perception of the stimuli may have been more than sufficient to make up for the difference. All the other subjects did actually more work, encumbered as they were in the ordinary experiments than when allowed their own rate as in the 'spontaneous' tests.[85]

Lastly there is still to demonstrate the fact that in the 'spontaneous' tests, a greater difference was exhibited between the flexor and extensor movements. Subject LO was the only person for whom the reverse was true. If we are allowed for the sake of convenience to talk of the general experiments with the rapidly alternating stimuli as "forced" work in contradistinction to the 'spontaneous' task where the subjects had more liberty, the inference is that if a certain goal is to be attained, a greater amount of effort will be put forth in the more difficult direction

[85] *Cf.* Dobri AVRAMOFF: Arbeit und Rhythmus, Philosophische Studien, vol. XVIII, 1903, p. 520. "Damit eine grosse Leistung eintritt, muss es bedingt, das *vorgeschr iebene* Tempo *schneller* sein als das selbstgewählte." Also p. 521-522 "Laut unseren obigen Ueberlegungen ist also der *Vorgeschriebene Takt unmittelbar nur geeignet, die Quantität der Arbeitsleistung zu erhöhen. Angenehmer,* wirkt aber das Selbstgewählte als das vorgeschriebene Tempo.

than would have been done otherwise. This, again, fits in with and corroborates the previous result, viz., that most subjects showed greater efficiency in the extra-stimulated reactions than in the auto-stimulated ones. In industrial affairs, this fact has been recognized long ago, as proven by the encouragement of piece-work under the contracting system in many plants and factories. Moreover, it is well known that trade unions are openly averse to this system, the question of piece-work pay or a weekly wage figuring greatly in recent strikes, notably in the tailoring trade.

A word or two to explain just how the comparison was instituted between the two sets of movements. In the 'spontaneous' records, the results are naturally in terms of achievement, while in the interference ('forced,' extra-stimulated) experiments, the results have been stated all along in terms of omissions. It was necessary, therefore, first to convert the omission percentages into those of achievement. The single omissions were given half values. Dividing, then, the extensor values by the flexor values, we obtain the ratio between the two movements in either set of experiments as follows:

TABLE XVIII.

Ratio Between Flexor and Extensor Movements.

Subject	Extra-stimulated	Auto-stimulated (spontaneous)
LV	92.11%	60.31%
LZ	79.56%	65.8 %
RO	89.%	78.43%
LL	100.2%	92.04%
FG	95.4%	92.7 %
SR	93.55%	92.2 %
ML	82.76%	81.53%
LO	79.51%	94.4 %
Average	89.01%	82.18%

CHAPTER VII.

Introspective Results.

The main problem before us here is to gain some analysis (1) of the antecedents of the inhibition and (2) of the state of mind during the inhibition. There are, however, many subsidiary points that bear on the work as a whole forming a sort of background or setting for the central issue.

A striking feature that comes to light in the introspection is the feeling of fatigue spoken of by nearly every subject. It is difficult to conceive that we are dealing here with a genuine fatigue factor, for considering that the whole working period lasted only 3 mins. and 18 secs. it is somewhat strange to find one subject (TR in XIII) say, "Fatigue comes in middle of 2nd revolution," i.e., in less than a minute after the work commenced. In spite of the fact that this fatigue is frequently localized, it seems as if it is more of a psychic origin. Many of us have probably experienced a feeling of faintness and fatigue at the very thought of a difficult and responsible task that must be done. Just as frequently do we experience the converse when on finishing some delicate piece of work after hours of great exertion, we feel so fresh and efficient that we could start on another task without any effort. Excessive tension or sudden relaxation are probably the partial causes of such states.

The attitude of the reagent (or agent in actual life) has much to do, it would appear, with the occurrence of the above-mentioned effect. The following bit of introspection from the same subject will make our point clearer.

Subject TR.

Series I, list XII. Rapid first.

"Omitted perhaps half.* Fatigue in arm, and work was increasingly bad. There is a decided tendency to give up and say

* This exaggeration which is found in the protocols of most of the subjects is very interesting. As a matter of fact T R omitted less than 1/5 in that particular list.

there is no use. Don't know of any mistakes I made outside of the omissions. Generally behind 4 or 5 and try to catch up, but as soon as the number is greater, I stop and go on to [react to] what I see. The fact that I tap from memory while looking at different dots confuses me somewhat. Not the least difficulty in association. Consciously stop sometimes. Attention on the stimulus not on the movement, not conscious of the movement until I get the pain in the arm from fatigue. Don't like the beginning of the next revolution. [In this list each revolution began with the close dots, thus calling for greater speed] and don't get keyed up for it. Keep a steady pace."

We see here that the subject thought her efforts were in vain, to begin with, and this thought evidently controlled her whole state of mind during the reaction. In lists X and XI where her "attention was not focussed on the work," she did much better work, and stated so in her introspection.

With the preceding, we might compare the attitude of subject LE who was almost in every respect different from TR, excepting the universal tendencies such as favoring the easier movement, exaggerating the poor quality of the work and the like.

Subject LE.

Series I, list XIII—rapid first.

"Easier than the one before. Don't think I had more than half right.* Very often the finger *would slip from the green* [difficult movement] *movement key to the red* [i.e., the downward reaction] and made a single red. Only muscular inhibitions. React to what is present. Don't try to catch up. . . . In seeing the fast at the beginning of the next revolution, I would do the slow more correctly, *then as the fast of the next revolution came up, I may have dropped the first 3 and started on a 4th.*"

The difference in attitude is even more strikingly brought out in the protocol of

Subject RO.

Series I, list VII—rapid first.

"Liked this series better than the first. It was easier. I knew that I could store up energy for the next revolution. There was

* In reality, it was only about 1/10 that she omitted.

catching up in the record because it gradually slowed down. Instead of feeling that I was going from bad to worse, I felt that I was going from bad to better. Attention on the movement. Omitted about 1/10 in the worst and 1/15 in the best revolution.** Omissions not so bad at beginning, then grew worse and towards the end diminished with practice. Maximal fatigue without offsetting by practice about the middle. I *get keyed up at the end of the revolution for the next.* No sensory confusion, but association apparatus grew weak when fatigued. In the last 2 [revolutions] the association apparatus grew better."

That the attitude[86] of the subject plays some part in the way of either inducing or warping the feeling of fatigue can be seen not only from the fact that sometimes there was more fatigue at the beginning of a record than towards the end, but also from the refreshed feeling with which the subject set to the second record at times. Encountering an obstacle, feeling that one is behind, in a word, the consciousness that one is not equal to the task[87] is liable to bring on not only an unpleasant feeling-tone,

** His omissions were less than 1/20.

[86] Seeing that the term "attitude" is fraught with so much ambiguity, a word of explanation might be in order. In the first place, then, it should be made clear that the "attitude," as understood here is not necessarily a *permanent* frame of mind taken up by the subject (*Stellungnahme*), nor is it employed in the more technical sense of *Bewusstseinslage.* It is somewhere between the two and represents a possibly changing frame of mind *affected by the situation and conditions faced.* To begin with, a certain attitude is called forth by the personal determinants, but an increasingly difficult situation, let us say, or perhaps the flush of continued success may change this attitude materially, so that it really depends on many factors, and is not entirely subjective, because it is modified and thus partially determined by the run of external conditions.

[87] What is here described as the negative attitude seems to correspond at least in its genesis, to MEUMANN's first type (*rückwirkende Tendenz*) of will-inhibition. Cf. his Vorlesungen, vol. I, pp. 637-638, 2nd ed. Considering, however, that subjects are apt, on occasion, to be possessed with a feeling of confidence as well as with a feeling of failure, though MEUMANN omits to mention this latter phase altogether, it is not easy to see why he should make out of this phenomenon a onesided affair, instead of giving it a generic place with positive and negative values. The concept of attitude is not only preferable for this reason, but also because it is in a way explanatory and takes us a step behind the inhibition especially as "nicht alle Individuen sind für diese Willenshemmungen gleich zugänglich, und wo sie auftreten, wirken sie sehr verschieden, loc. cit. p. 639.

but even the feeling and sensations of fatigue. On any other view, it would be difficult to explain and reconcile certain statements that come up repeatedly in the introspective accounts.[88] Samples of this might be found in the protocols of FG and ML.

Subject FG.

Series 1,—List XII—rapid first.

"Did it better [than the slow-rapid list]. Think this the easier. In second revolution I got mixed up and alternated the wrong way. Looked out for this and got mixed up only once. *Suddenly all my tension relaxes. Feel comfortable* and the speed is slower."

Subject ML.

Series I,—List IX—slow first.

"Thought I only left out about ten dots in the last revolution. Improved towards the end. There was eye strain and *everything seemed blurred in the middle revolution.* There was one place in the middle of each revolution when association would be apt to break down, sometimes only slightly. *Generally speaking the curve of work went up* though irregularly. Fatigue *set in at the very start, but soon this fatigue was underneath.* It was pleasant to finish up the end of one revolution and did pretty well in the last bunch, and then relaxed so much that I had some difficulty in keeping up with the slow ones. Preferred gradual keying up."

[88] It is gratifying to see that Ladd and Woodworth (Elements of Physiological Psychology, p. 540) are careful enough to say on this point: "Inasmuch as a short series is relatively free from inner interferences whereas a long series involves many such chances, a short series is apt to be performed with better success than a long series. The appearance then is that fatigue has influenced the longer performance. But *since* even this long performance need be only a few seconds in length, and since recovery from the condition is prompt, such fatigue can scarcely be identified with the progressive and metabolic type." Our results are certainly corroborative of the further statement that "there is apparently a radical difference between metabolic fatigue which really lowers the power of an organ and the inhibitions which result from the action of certain stimuli to the organ. *It is probable that inhibitions or interferences give the key to most appearances of intellectual fatigue.*" It is well worth underlining the last sentence considering that the term fatigue had beclouded a number of issues in psychology that are either antecedents of fatigue or else only remotely related to it.

The blurring effect is an important point especially because it is not a rare occurrence. Several subjects have spoken of sensory inhibition, and a few others stated that the stimuli they didn't react to were not perceived in the first place. But why should some dots be perceived distinctly and others not? Here we come upon a matter of great theoretical importance. From many protocols it may be gathered that the distinct perception of the stimuli depends in a large measure on the quality of the immediately preceding reactions. If all goes well, there is no blurring of the stimuli. If the subject, however, had been making mistakes or was lapsing into a state of confusion, the oncoming stimuli would make but a faint impression. It would seem as if in accordance with the Action theory, the motor difficulties produce a blocking of certain higher channels that are involved in apperception and perception. This is by no means an attempt to go into the merits of the various inhibition theories that are in the field at present, but it is well to keep in mind the theoretical bearing of the blurring effect.

Method of Reaction

The introspection reveals that not only are there individual differences in the method of going about the work, but that every subject changes his attitude and method more than once in the course of the reactions. The rate of speed, and the direction of change, i.e., whether the dots are in an ascending order of speed or in a descending order, and sometimes even their number-arrangement—all these factors call forth different determining tendencies. The subject may have been doing one thing for a short period, and a few moments later, he may have adopted a different course entirely. The change was not a chance happening or an arbitrary move. It was brought about by an altered situation.

The most remarkable instance of this transformed attitude was evidenced in going over from the "rapid-slow" to the "slow-rapid" list, or *vice versa,** but reserving the discussion of that point to another section, we have other instances of this fact.

* Vide sub "Application to Pedagogy."

bobbing up again and again in the protocols. Here is a good example:

Subject GD.

Series II—list XV A.—slow first.

"Liked this much better. In the last record [the list was the same, but reversed, so that the rate of speed was in a descending order, i.e., it began with the very close dots] the whole thing seemed to be just as fast as the fast part of this. The first part here seemed slower than the slow in the experiment before. Only twice I found myself waiting for the next dot. *There would be a tendency to alternate during this waiting. When it was going very fast and I was hurried, I would have the tendency to continue the same process. When going very slow I do the dots singly.* When a group is all gone and out of sight I don't try to catch up."

Subject FG.

Series I—list XI—rapid first.

"When I do good work, *it seems to go more slowly.** Made several omissions and one mistake. Had to catch up. Anticipate when seeing the beginning of next revolution and get keyed up. Can count the number of taps by the sound."

Series I—list X—slow first.

"Liked the one before much better. The grouping in this was such that I couldn't keep up with it. It seemed to be a different one [list] from the one before.* Didn't like the side movement. There was inhibition with the green [stimulus for side movement]. Found myself working at wrong dots in this. For a whole string I did the wrong ones and then had to stop and begin afresh. Worst work was in the middle revolution. Then I started improving."

* The *speed illusion* is one of the most interesting results in our experiments, especially because of its universality; and forms a valuable datum for discussion in dealing with time perception. The explanation of this illusion, which, so far as the writer is aware, has not been hitherto mentioned, seems to lie in the increased tension due to the unrealized impulses.

* Subject F G was the only one who knew that the second list given in the sitting was the reverse of the first. As the large screen in front of the stimulus-cylinder concealed the manipulation of the apparatus, no subject had the opportunity of obtaining this information, except by being told.

Subject RO.

Series II—XXII A.—28″ slow first.

"Attempt every group now but don't attempt to complete it as I used to do before [when rate was not as high]. This fights off the inhibition. The inhibition used to be between intention to do, say, six taps and intention to react on the oncoming stimulus. Here the whole emphasis is on the advance work. My setting is for the new color always."

Series II—XVIII A.—28″ slow first.

"As soon as I felt the lack of control, there was a tendency to relax. As soon as memory images faded out, I let the whole thing slide until I could get another grip."

Subject SR.

Series II—XVIII A.—slow first.

"Contention of impulses in the form of images [did not state what kind] and conscious of both impulses and of the fact that there is a contention. What settles the struggle is complete banishment of both impulses from consciousness. This is voluntary. Yield to the new impulses coming."

Owing to the play of several different tendencies during the sitting, the subjects' statements appear at times contradictory, but it has already been suggested that the methods adopted vary with the course of the work. Subject GD in mentioning the two conflicting tendencies that he experienced made it plain that when he is hurried, he is inclined to continue in the same process instead of alternating in accordance with the stimuli, while in waiting for a dot to appear he has a tendency to alternate. Subjects FG and SR are not as definite. The former tells us that there was a tendency to alternate (with XIV rapid first, which is the easier list for him) and after the next record (XV, slow first) says, "Tendency to continue in the same process, which I had to control"; while the latter (IV 24″) makes it known that there was a "slight tendency to alternate" and almost in the same breath adds "also a tendency to keep on striking the same key."

If the alternation tendency is at work during a moment of waiting it might well be due to anticipation. Another possibil-

ity suggests itself quite plausibly. The subject may have experienced the alternation tendency while performing the more difficult movement, whereas the continuation tendency may have forced itself upon him while making the easier reaction. In general the latter tendency was more in evidence, and a distinct effort had to be made to control it.

The Lessening of the Task.

In agreement with what has been said before on the *course of least exertion,* the introspective protocols disclose numerous ways of making compromises between the task and the response. It has already been stated that various methods are adopted to suit various moments of the changing situation. The shunting is systematic. The subject has to omit, but implicitly his task calls for an effort to react to every stimulus. It is clear that he cannot proceed and carry out the burdensome instruction in all that it implies. The only course open then is to make allowance in certain cases or to exaggerate one element of the task at the expense of another (compensation). What happens is this: The specific determining tendencies, which vary according to the personal determinants, come into play to counteract the primary or dominant determining tendency. If the latter tendency were to prevail without repression, the subject would be going along at a comfortable pace; but he feels a certain responsibility and this determines him, let us say, to keep on tapping. He does it, however, aimlessly without reference to the stimuli. This is one way of discharging his duty. Now there are many other ways that will be gone into more fully in the chapter on individual differences. Anticipation seems to be resorted to quite frequently, but anticipation of a particular sort. The subject, viz., makes a deposit of a single or double reaction during a period of waiting, and counts it afterwards as belonging to the oncoming group (Subject R U especially is given to this. In Series III (XXI B. 28″ slow first), he says,

"Tendency to continue red, and then count it as the first of a group that hadn't come up yet, i.e., anticipated."

This easing up in attitude is more articulate with other subjects, notably TR, SR and LV. The latter tells us

"I don't appear to see all, but only as much as I can do. Voluntarily omit those that are too much. . . . 'That's superfluous,' 'Two is enough from those there' seem to suggest themselves on the motor side, so my apperception is reduced."

Series III—XVII B.—slow first.

Or again,

"Had a feeling of cutting part off and throwing it away. There was hardly any conflict of impulses. The frame of mind is 'Get what you can and leave the others.' "

Series II—XXIII A.—rapid first.

Similarly subject SR had a tendency "to stick out certain groups only" and take a lax attitude. Said

" 'What's the use of trying to catch up.' Apperception weaker for the dots I did not pick out and stronger for those I did."

Several subjects had a tendency to rest satisfied with making a single reaction instead of a double reaction. That was especially the case with the more difficult movement, as may be seen from the tables in a previous chapter. Subject ML states

"would stop short the impulse to react on green [upward movement] every time I'd see a red [downward movement] coming up" (Series IV, xviii c, slow first).

There were also such occasional lapses as these: striking a key with two fingers, striking the two keys with the easier finger, using the knuckle instead of the finger tip, stopping prematurely at the end of a revolution. These lapses are recorded of subjects SR, ML, RO, and FG.

Another subterfuge seemed to be the omission of isolated stimuli. It was the group of two or three that received most attention, except by three subjects. (Of these two regarded the single dot or cross as a group, i.e., to them it was the alternation in color that formed the group.) The reason for emphasizing the larger groups is obvious. There are really two causes for that tendency. In the first place, the larger group is, as far as most of the subjects are concerned, apperceived with greater vividness. The large group makes more of an impression. Secondly, there is greater economy of effort in reacting to three stimuli of the same kind than in alternating from one key to the

other. In repeating the same reaction, instead of changing the movements, there is also some time saved.

INHIBITION

We now come to a central point, viz., that of inhibition. To begin with, we must realize that inhibition is but a name for a number of phenomena that have at their root a similar process. In all sorts of inhibition we find one salient feature that really determines their coming under this category, and that is the *checking process*. That is true both of physiological and mental or psychological inhibition. With the former we are at present not concerned. The latter, however, does need some further analysis and classification. In speaking of psychological inhibition one is apt to confuse the cause and the effect, the state of inhibition and its antecedents or else the state proper and the further results. In a sense, every state of consciousness is an inhibition inasmuch as it holds other states out of consciousness. A moment of concentration is an inhibitory state on that score. It is clear, however, that such a broad view would not be of much use, and for technical purposes, it would be but a mere quibble. We must distinguish between the feeling of tension that leads to inhibition and the mental content that represents the latter state. In the same way it is necessary to mark off inhibition from reversals, though lapsing into wrong movements is often the effect of inhibition.

In our experiments, the subjects distinguished three kinds of inhibition. The first was spoken of as sensory and was described as a blurring effect of the stimuli. There seemed to be also a couple of variations in this type, (1) in distinct perception of the stimuli; (2) after-images or "overlapping of visual content" as subject LV expressed his experience on several occasions. In his case, the after-images were of the stimuli representing the more difficult movement.

The second type of inhibition was sometimes called "muscular," sometimes "motor"; and by that the subjects seemed to wish to convey the idea that this inhibition had to do with the realization of the impulse. They associated it with the muscle

because they felt it checked them when they were nearer their goal. This type did not appear to be so serious as what they called the "Associative" or "Central"[89] inhibition. It was really the last type that caused most of the mischief, the disturbance taking deeper root and lasting for a greater period of time than with the other two kinds. The associative inhibition frequently brings on the sensory kind, while the associative kind may be the result of the purely motor type, but sensory inhibition never appears first, i.e., there is no blurring effect until the difficulties begin to accumulate. It is probably then, too, that the ignoring of parts of the stimuli-series begins.

The Apperception of Groups.

The grouping of the dots seems to be an adaptive device making for greater speed. The higher the rate of speed, the greater is the tendency to regard two or three stimuli of the same kind as a single unit. This is apparent not only from the fact that the slow reagents are apt to show this tendency more than the quick reagents, but also from the two different methods of attack frequently employed by the same subject.

In the section on Reaction Method, we have already found that subject GD would do the dots singly at the slow rate. Other subjects seem to have adopted the same method.

"At the lower speed, I take the dots individually. At the high speed only I group the dots. The grouping is a device to keep up" (LO in XXI A).

Subject LZ, who was the slowest reagent of all, had no opportunity, at any time, to react upon each dot individually. He therefore takes them

"In groups, and at the high speed groups the groups, so that several groups make one group" (XX A).

This tendency may be compared with the apperceptive tendency in reading: Reading at leisure is, in the writer's experi-

[89] These terms were used by the subjects only as convenient expressions and by no means as theoretical suggestions. After the three kinds of inhibitory experiences had been distinguished, it was agreed between subjects and experimenter to understand by such and such a term a particular experience. The subjects employed their own terms; hence the synonyms we find above.

ence, done mostly in phrases and sometimes even in single words. Very often this is a hindrance in following the continuity of the sentence or paragraph. In spending too much time on the single words we lose the relation or drift of the whole. Many of us get more out of our reading when we do it while in a hurry,[90] perhaps when we are anxious to finish the chapter before the library closes, or when, in the margin of consciousness we feel the immediate pressure of other and more important work. It is then that we take in at a glance whole clauses and sometimes even sentences,[91] by falling into a certain galloping tempo that accents the important and skips the unimportant parts of the sentence, or in the language of JAMES

"We give attention only to substantive starting points, turning points and conclusions here and there."[92]

The significance of a group of dots cannot, of course, possess the same significance as a phrase or a clause, yet subject LO who takes

"The dots in groups, especially when they are going fast" gets them "as letters of a telegraph message."

The meaning of the stimuli is constituted by their number, color and shape and possibly also their sequence. The larger the group, the more meaning it has. Also the stimulus of the color and shape recognized as requiring the easier movement is more meaningful than its opposite.

The connection betweeen the apperceptive tendency and the course of least exertion will be brought out in greater relief when

[90] Cf. TITCHENER: Experimental Psychology of the Thought Processes p. 204; "It is surprising how accurate an impression may be gained by hurried selective reading, 'skimming,' if only one has had sufficient practice."

[91] This of course is outside the question of fixation pauses and eye movements.

[92] James: Principles of Psychology, vol. I, p. 265. So far writers on the psychology of reading have taken up differences in individual types or differences in material as affecting the rate of reading. From observation, however, we know for certain that the rate varies not only with the individual and with the material, but with the purpose in reading, as Titchener points out in his own case (loc. cit. p. 203); and we might add: with the attitude of and conditions imposed upon the reader. Here, indeed, lies a problem in reading for future investigations.

we come to discuss the difference between sensory inhibition of the RANSCHBURG type and motor inhibition.

In order to get at the inhibitory state of mind more closely, it is necessary to start with the antecedents. Generally speaking, inhibition is first caused by a conflict between the impulse to react on a previous stimulus and that of responding to one that has just come into sight, the resulting inhibition may be slight and affect only two or three stimuli. It may consist in the inability of stopping one impulse in time to start for another response. But it may also go deeper than that and throw the subject into a daze. *The degree of inhibition may be measured by the degree of definiteness the subject has of the instructions. That is clear from the accounts to follow.* Inhibition, also has more chances of coming on, the more intent the subject is on his work. Subject F G generally had his attention off his work. The instructions were held in marginal consciousness and he often lapsed into reversals, striking the wrong keys, but as already remarked, that could not be called inhibition. It was only when he caught himself reacting wrongly that he was inhibited and had to stop in order to start anew. The wavering or shifting of attention seems to be the sole cause of reversals, and the reversals eventually give rise to inhibition, i.e., as soon as the subject becomes aware of his mistake.

The most characteristic thing about inhibition is the feeling of helplessness in various degrees. Here is how subject L V describes the rise of his inhibitions:

(Series III—XX B.—rapid first.)

"Hand and eye seem to be entirely disconnected when during slow part of list I made needless mistakes. Feeling of astonishment that I should do so poorly at slow rate. First revolution was about 90% right. Second made me shift and put me all out. *I knew I was making reversals and could do nothing.* Internal speech "no" and feeling of ostracism, of self-depreciation I was in the state that I couldn't believe I had done it. . . . There was tension and inner exhortation to do better and feeling as if I were forming a new habit."

In the second record he says

"Consciousness of omissions great in second revolution there

was a reverberation of the omission consciousness, which was turned into reversal. Felt I had omitted and then turned back to make up, with the result of making a reversal. Looked deliberately at red and tapped green [struck the key corresponding to the green dot]. Disparateness of eye and hand. Hand seems to be refractory to decision." In another variation (series IV, XX C, rapid first) it is stated that "sometimes my fingers would stick in the middle of a series of dots, and then I set out to finish it, but different groups came up and then I was helpless; couldn't change right off."

With subject L Z, it was a case of knowing "I have to move a finger, but not exactly the associative arrangement." (Series IV, 1 XVIII C., slow first). Subject R U

"Had a number of omissions, and times when I felt helpless from lack of gathering myself together. That was, however more at the end when I was more fatigued. Then it was just as a red group and a green group inhibited each other." (Series III, XVIII B., slow first.)

By way of parenthesis, and in order to bring into relief the non-inhibitory attitude, let us quote what the same subject reports on the second record that morning, i.e., when the same list was reversed, requiring the most rapid work first. We should expect to find the subject here under the effect of fatigue just as he reported it a few minutes before but *mirabile dictu* he found it "very much easier," in fact, "twice as good" [his objective results bear this out. In the first, there were 32 double omissions and 7 single omissions for the easier movement, i.e., the middle finger, and 48 double omissions and 6 single omissions for the more difficult movement, executed with the ring finger, while in the second record there were 16 double omissions for the one and 29 double and 2 single omissions for the other.]

"Did first revolution perfectly. Had a number of omissions, but no reversals. Easier to start with a color,* I last hit [reacted to] and this is usually red. Tendency to continue a red or a green according as I tapped before. While *I was waiting during a break, my fingers would react automatically to a dot that*

* It will be noticed that most of the subjects refer to their movements always in terms of the stimulus, probably because the movements were varied while the stimuli remained the same throughout all the experiments both as to form and color.

had passed without real apperception. Notice that I reacted only after finger was down."

The weakening of the determining tendency during inhibition is attributed sometimes to faint apperception, but faint apperception in itself is already a symptom of inhibition and not a cause. The cause lies in a lack of coordination between two impulses where the immediate one cannot find a proper outlet because a determining tendency presiding over another impulse had inserted itself and had set off a more spontaneous impulse.* Meanwhile the apperceptive impulse, i.e., the impulse to react on the very stimulus that is perceived at that moment, and which, under ordinary circumstances, would be immediately executed, is blocked. It is this incident that heightens the state of inhibition. The confusion arising from this state may immediately lead to a break, with the subject doing nothing and his mind in a daze, or else the vague and general determining tendency may be present to react somehow. In this state the subjects react at random, the stimuli having little or no meaning for them. In the former event, the break is much longer, but in both cases, a distinct effort must be made anew, as the determining tendencies reinstate themselves, and only then is the coordination between perception of dots, association of movements and motor ability formed again.

To illustrate from the protocols some of the different stages of inhibition together with their antecedents: Subject R O says

"Felt an impulse to react and there was no outlet. There was a perception of red and green, but no apperception. Then my own muscular inability saved me, i.e., I took only a certain number of the greens. All the difficulty came with the greens" (Series IV, l. XX C, rapid first).

* Cf. e.g., "I hit a color I didn't mean to hit. Internal speech [with the first] and in spite I hit the other." (RU in XXII C, slow first 28") "Felt a clashing of impulses, almost a muscular feeling of pull to keep on striking the green that I had been striking." (SR in XXI C Aux) "Felt an Impulse of tumbling over. Stimulus was green, and had the impulse to strike red." (LE in XXI C Aux) "When I hit a wrong key, I instantly see that I am wrong, simultaneously with my finger on it, but I can't help it. Once I found myself going wrong, but was helpless and that made me omit the next one" (RC in IV, 24")

Subject L E was

"Worn out half way. Had random tapping. Stimulus not apperceived very well. That's the reason for the random tapping. Apperception was only of form not of color . . . one associative breakdown, and felt that it wasn't due to fatigue. I saw it was green and saw the red, but my muscles didn't work correspondingly, but reversed." (Series IV, 1 XXII, C, rapid first) "When I omit, I see the stimulus, but the impulse is not there, I don't associate the dot with the movement. There is a stupor." (Series II, 1. XVIII A, slow first.)

Subject L L

"Suddenly I see I'm wrong. If there are two or three red dots to tap and can't get them tapped before they disappear, I find myself tapping with forefinger [easier movement] even when a green dot comes. I can't stop the impulse, once it's given, and if I do succeed in doing it, the result is that I have a motor inhibition and can't do anything. . . . After the motor break there is a period when I can't associate the dots with the movement" (Series II, xxi A, slow first). "Realized what I ought to be doing, but would go wrong just the same. It comes on in the high speed" (Series II, 1. xx, rapid first). "When I can't keep up, there seem to be two difficulties. The one is to alternate the impulse, the other is to keep the fingers going fast enough." (Series II, xix A, rapid first).

Subject L O

"Confusion brought about because I start to execute a movement, say with the forefinger, then green dots come up, and I either go wrong tapping out the red [with the forefinger] before [I change], or else am altogether inhibited and don't tap at all." (Series II, xx A, rapid first).

Subject M L

"With inhibition, I had a blank, and feeling of futility marked by organic sensations and strain in fingers and discomfort in hand. Many times I didn't apperceive the significance of the different dots. The association of the movement and the dots was lacking." (Series II, 1. xviii, slow first) "Inhibition would follow the conflict between finger work and apperceived dots. Prepared *for a certain kind of motor* discharge, and this worked itself out, even though I ought to change." (Series II, 1. xix, rapid first.)

Subject R O

"Inhibition to-day was on the sensory side. The fast revolving dots were not apperceived. Caught up from memory part, and part I let go. What I caught up, I reacted to without inhibition, and what was seen during the reaction, did not affect me any more than it was blank paper." (Series II, I. xx, rapid first.)

Subject S R

"Inhibition caused by the tendency to alternate from one color to the other. Now before I strike up the whole group, another group of the same color is seen, and then there is a real conflict between impulse to react to present stimulus and that to react from memory. This causes an inhibition, a sort of neutralizing effect of the two impulses. Then consciousness of confusion; then comes the consciousness that I ought to be striking, and that memory brings on the unpleasant feeling-tone. During the state of confusion, I don't perceive the dots running by at all. (Series II, I. xxiii, rapid first.)

Subject V L

"Not able to stop an impulse when other dots are coming in. You know exactly what you ought to do, but can't help it. Feel helpless. Feel as if consciousness had taken in only a fraction of the whole. Most noticeable element in the bodily concomitant of the inhibition is a tetanus. You feel as if you had worked harder than necessary. Hands rigid after inhibition. The *physical* concomitant is crampedness and warmth in hand. The *conscious* side consists of overlapping fields of consciousness as if the 6 times tapping overlaps with 4 and 2 other coming up. It's a feeling of nothing rather than of something. It seems as if the conceptual or perceptual goes off into a physical recognition of failure. It's a *finger* feeling of failure. (Series II, I. xxii, A, slow first).

In the accounts of subjects L O, R U, and R O we find a description of the more advanced stages of inhibition. The former "felt paralyzed just as if I wanted to wake in my sleep. Upward motion very fatiguing. Had a feeling of powerlessness. Was almost giving it up." (Series IV, I. xxc, rapid first).
In the second record he

"was dazed and simply alternated at random."

In another record he thought

"it was all random tapping. 3 or 4 times I thought I'd give it

all up. Complete inability to follow the stimulus. Plenty of muscular inhibition. Also associative. The whole thing was too fast. Was so confused that I hadn't looked at the dots at all" (Series IV, I. xviii c, slow first).

And again

"Very distressing experience. Not quite as fatiguing as before. General confusion all around. It isn't confusion in intelligence. It's confusion in volition" (Series III, xv B, slow first).

Subject R O gives a more definite account of the different stages of inhibition that he experienced. Once (Series IV, I. xx c, rapid first) he reports.

"One blank space owing to muscular fatigue. Inability to react for one group lowered my tonicity. The whole motor tonus was so low that I couldn't react, although the association was there, but less vivid."

Further (Series IV, I. xxi c, slow first

"Instructions less definite in my mind. Instructions took on the form of 'Move' rather than 'move in a certain way.' Everything obscure, then. . . . Less effort to apperceive distinctly."

Early in the year this subject had marked off three stages of inhibitions where the first began with a "tendency to alternate with increasing speed"; the second stage, "when I did not complete the movement"; and in third stage "I simply did nothing, i.e., I wasn't tapping. That happened only once" (Series I, I. vi, slow first). The movements in that series were the vertical and the lateral reactions.

We find another version of this stratification. One protocol has the stages classified after this fashion:

(1) "When I try to finish up from memory," (2) "When new dots crowd out the old, and I allow myself to be distracted," (3) "A jumble when there is a batch of green-red and red-green with no definite number in group. Then there is a tendency to alternate [blindly]." (Series II, I. xvi A, rapid first, 28.")
From the point of view of control, the stages are given as

(1) "perfect control," (2) "when reacting from memory crowds out the new ones [stimuli]," (3) "when the new stimuli crowd out the old stimuli," (4) "'complete confusion." (Series II, xvii A, slow first).

The week after, he states:

"Didn't feel the four stages as last time. As soon as I felt the lack of control, there was a tendency to relax. As soon as memory images faded out, I let the whole thing slide until I could get another grip. As for inhibition itself, there was a decided sensory inhibition when I was trying to catch up from memory. Tendency to continue the same [scheme of] movements when I lost the sensory cue and was tapping at random." (Series II, xviii A, slow first).

Of a more serious kind is the point

"of inanition when I saw the thing going by and couldn't catch up. It was no conflict of the one inhibiting the other, but the whole impression was inhibitory, where I couldn't respond at all."

With the increasing difficulty of the variations, the latter type of inhibition occurred more frequently, while the milder form came out in several new ways. Towards the end of the year, with the most difficult movements of all the series (upward and downward reactions), he reported

"A less advanced stage of inhibition centered round the red (downward movement). Red so easy that I let it take care of itself, and attention was centered on the green (upward or extensor movement), so that whenever in doubt, I got to the upper and shunted off the lower" [confirmed by the objective results].

The more deep-seated kind consisted of a

"type of general inhibition when I even lost the determining tendency. Saw the thing going round, and the only mental counterpart was the unpleasant feeling that I should have been reacting."

A. Retroactive Inhibition of Determining Tendencies

On examining the foregoing and other accounts, there can be no doubt that the principle or law of retroactive inhibition*

* On first thought the term *retrograde inhibition* might appear more appropriate in this connection than the designation *retroactive inhibition,* but on careful analysis it will be seen that, as in the case of memory, the cause of the decline is the *increased burden of the task.* In the memory experiments of Müller and Pilzecker this accumulating impediment was the *nachgeschickte Tätigkeit;* in our problem it is the *increased speed,* the *sustained effort* and *concentrated attention* taxing the reactive functions of the subject as the work during the sitting progresses.

is operative in the interference of will-impulses just as it is a factor in the interference of associative and reproductive tendencies. The application is somewhat different, however, in our experiments. *It is the most specific determining tendency that is the first to wane in inhibition.* Speaking more concretely, the course of inhibition may be traced as follows: instead of executing a particular sort of movement at the moment a particular stimulus is perceived, which would be the tendency operating if everything went without a hitch, the subject is pulled in two different directions with the result that (1) the most specific determining tendency, just because it depends entirely on the momentary coordination between eye and finger, loses its hold on him. What happens next? The subject has not stopped reacting. The *Aufgabe* is still before him, and that too in a specific form. He may omit a movement here and there. His reactions may be retarded, and he may be occupied partly in catching up with a group that had just passed by; (2) nevertheless he still associates the red stimulus with one type of movement and the green with another, and proceeds, amidst occasional omissions, to execute these movements. In this stage, there is no synchronous coordination between stimulus and reaction. The muscles cannot obey the impulse quickly enough, and as other impulses keep rushing on, some of the movements naturally remain unexecuted. Hence the subjects refer to this sort of inhibition as "muscular" or "motor."

The next step is somewhat of a digression. At least it forms a dividing line between the foregoing stage of inhibition and the following stages. When the subject reacts with one sort of movement to a given stimulus and with another kind of movement in response to a different stimulus, but has at the outset lost the proper association, he is still performing his task, though the details of the instructions are not clearly in mind. γ There is still a tendency to correlate a change in color with a change in movement. The correlation extends also to the number of the stimuli. Thus we see a certain amount of association is involved in *reversal,* but it is not an association that requires very much attention. Ordinarily, the subject is not aware of any inhibition

during reversal. In fact his mind is scarcely on the work, except in a marginal way. It is only when *he catches himself reversing* that he becomes inhibited. With some subjects reversal is very rare.

We now come to the more advanced types of inhibition. As the difficulties increase, the subject becomes less and less aware of the meaning of the stimuli, but there is still (4) a tendency to react, even if it is "wild tapping" as one subject expressed it. This random reacting shows, at any rate, that the *Aufgabe* is still influencing the subject; and the fact that, though carried on regardless of the stimuli, it includes alternation and is not confined to striking simply one key bears evidence that the determining tendency presiding over the alternation of movement is not in abeyance. Lastly we come to the stage when the subject's mind is cognitively[93] almost a blank. (5) There is no action at all, and whatever mental content is present belongs to the affective sort (perhaps a complex of feeling-tone, *Bewusstseinslagen* and emotion).

If on the basis of this analysis we were required to label the different determining tendencies for convenience sake, we should name (1) the most specific of these—the coordinative determining tendency, then in the order of their disappearance, (2) the apperceptive,[94] (3) the correlative, (4) the perceptive and (5)

[93] This word is used here advisedly, for the writer is of the same opinion as Colvin who maintains that the degree of *cognitive clearness* does not go hand in hand with the degree of *affective intensity*. To him they are rather mutually exclusive. At least, the one is in the focus while the other is in the background of consciousness. Too often we are prone to mistake indefiniteness and vagueness for absence. There may or there may not obtain that see-saw relation between the cognitive and the affective in consciousness, but of one thing we are certain, viz., that clearness or definiteness is not our only indication of the presence of mental content. But cf. Colvin's illuminating treatment of the cognitive and affective elements in attention in The Learning Process, pp. 255-259.

[94] There are a number of psychologists who avoid the term apperception because of its speculative coloring, and as they find it evidently difficult to replace it entirely by the word perception, they employ the term "apprehension," which in the opinion of the writer is much less satisfactory than apperception. Let it be stated here that "apperceptive" as used here refers not only to the perception of the stimuli, but to their meaning as calling for a definite response.

the executive determining tendency. When all of these are hibited, the *Aufgabe* may still be said to be present in the negat affective tone which accumulates with the increasing inhibitio

B. AFFECTIVE TONE AND ORGANIC CONCOMITANTS OF OF INHIBITION.

The affective complex connected with the inhibition may v from a feeling (or *Bewusstseinslage*) of resignation to emotional touch of shame and self-depreciation. It must no understood that there is a feeling-tone with inhibition o Throughout the work there are all sorts of affective compl with most of the subjects, but they become most marked du the inhibitory state, and tend to prolong this state. It is diffi to separate out the various elements of the affective com which comprises generally organic sensations as well, but protocols contain a large number of references to "sinking i ings," "'feelings of ostracism, of dissatisfaction, discomfort relief, suspense, paralysis, confusion, resignation, distressing perience, shame, anger, impatience, disgust, irritation, despair," besides those put in the form of a metaphor.

The organic concomitants of inhibition are of greater inte to us, because they help to throw some light on the question a whether inhibition is a positive or a negative state, i.e., i marked by certain features of its own, or does it become kn by the absence of these features and by a suspension of previo experienced activity. Introspection points to the former hyp esis as the true one, though we must be on our guard aga considering all of the affective complexes and organic sensat as counterparts of the inhibitory state. Some of them ma antecedents, others may be the effects of the inhibitory consci ness. To determine the time-relation with anything like cision, it would seem to us, is an impossibility, but since no ments other than these were observed by the subjects, on justified in tentatively building up the state of inhibition ou these various complexes, some of which have already been c and others (organic sensations and complexes) are to follow

To begin with, strain sensations and tension in fingers seem to be general with the subjects, often developing into fatigue.

"Feel kinaesthetic sensations; line of tension from wrist to forearm and sensations of a similar quality behind the neck and verbal commentary that those sensations were there." (L O in xxiii C. Aux.) "Noticed a great deal of tension. Thing most prominent was effort to make finger move at all" (L L in xix C aux.). "Aware of muscular sensations. Had inner speech" (R U in xix C aux.). "Organic sensations in lower part of abdomen. Impatience and tendency to jump up and hold back the dots" (A B in xxi C.)

"Aware of only organic sensations. Experienced tightness in chest and breathlessness just as when you feel that you are in a tight fix and had to do something you can't do; and something of the same tension in fingers. Peculiar feeling of tension about forehead." (L O in xxi B aux.). "Feeling of tension. Kinaesthesis in general, but more localized in arm and back of neck" (R O in xxi C aux.). "Feeling of tension of upward movement. . . . Attitude becomes more tense, rigidity in back" (M L in xxiii C aux.). "Had attention on finger and kinaesthetic feeling. First apperception, and then dot wouldn't associate with the movement. The only way to do is then to stop. Then fully conscious of movements down the body through the fingers." F G in xviii A).

This leads us to another important point, viz., the diffusion of the impulse and the resulting phenomenon of what we should call

SYNKINESIS.

The term as employed here includes not only actual movements of other parts of the body than those necessarily involved in the performance of a given reaction, but also kinaesthesis in such other organs.

The spread of the motor impulse has been observed by numerous investigators in various connections. *Exner,*[95] as early as 1873, notes the spread and intensification of the motor impulse under excitement, with the result that the reaction-time of the subject is shortened. W. W. DAVIS[96] has found that activities

[95] Exner: Experimentelle Untersuchung der einfachsten Psychischen Prozesse. Archiv f. die gesamte Phys. (Pflueger) vol. VII. '73, p. 619.

[96] W. W. Davis: Researches in Cross-Education. Studies Yale Psychological Laboratory VI, pp. 48 and 49, and VIII, pp. 106 and 107.

involving certain muscles are transferred to other muscles, an uses these results, along with those of other investigators, t prove the possibility of transference in training. *Wissler* an *Richardson*[97] give an account of some experiments "with the: diffused movements in normal unfatigued activity of the an muscles" from which they infer that "diffusion takes place i both inward and outward directions"[98] *Féré*[99] has collected number of facts, both from his own results and those of other to show that the exercise of a specific set of muscles affects oth members of the body as well, and to some of these phenomer that are characterized by great agitation generally, he applies tl term "*ivresse motrice.*"[100] In LANGFELD's work on finger mov ments, the spread of the impulse is marked by the tendency move the neighboring finger when the extensor muscle of the i dex finger is innervated.[101]

It is not necessary to dwell any longer on the literature on th subject. That the motor impulse spreads under certain cond tions is an established fact.[102] It has been demonstrated in diffe ent phases of muscle-activity, but so far as the writer is awar it has not been connected directly with inhibition.[103] A faint su gestion in that direction might possibly be had in T. V. MOORE "Study in Reaction Time and Movement," Psychological Revie

[97] C. W. Wissler and W. W. Richardson: Diffusion of the Motor Ir pulse. Psychol. Review, vol. VII, 1900, p. 29.

[98] Loc. cit. p. 35.

[99] Ch. Féré: Travail et Plaisir. Chaps. XXVIII and XXXI.

[100] Loc. cit. Chapter XXVII.

[101] H. S. Langfeld: Facilitation and Inhibition of Motor Impulses, Psyc Review, XXII, 1915, p. 472 and p. 474.

[102] MUENSTERBERG in his discussion of *Mitbewegungen* (Beitraege zur E: perimentellen Psychologie, Heft IV, p. 192 ff.) does not question the occu rence of irradiation, but seems rather to accept it as a matter of fact. Wh he does contend against is the exaggerated doctrine of symmetrical bilate ality as an original and universal tendency.

[103] Cf. also JUDD, MCALLISTER and STEELE (Analysis of Reaction Mov ments. Psych. Review, Monograph Supplements, vol. VII, 1905-06) wl explain preparatory antagonistic reactions, i.e., the tendency on the part the reagent to press down the key suddenly before releasing it—a phenom non already reported by W. G. Smith in Mind N. S. XII, 1903,—by a tributing it to motor diffusion. (loc. cit. p. 166, 182-183.)

Monograph Supplements, vol. VI, p. 59. "For a slower movement . . . dissipated along the wrong paths."

In presenting the following excerpts from our protocols, we shall have to view the matter from a somewhat different angle. The question before us is whether such synkinesis is an asset or liability to the reagent? Most of the cases that have been cited in the literature tend to prove that synkinesis is facilitatory; and it is conceivable why that should be so in a great many instances. But we must not infer that synkinesis exercises invariably a facilitatory influence over the members or muscles actually involved in the activity. Sometimes the spread of the impulse and the resulting general excitement are symptomatic of the inhibitory state and the tension and strain connected with the work, e.g., when there was

"a great deal of excitement, breathing faster, general tension in left foot and arm [ipsilateral] general tension in right hand and some movement [contralateral]. A lot in feet. Tendency to run wild, go up and down and lose counting. Speed was too strong. Have spoken of having tension in right arm before, but it was more pronounced this time" (L L in xxiii C aux.)

Sometimes a transference of this sort did not go together with great excitement, and probably betrayed a tendency to rest he one hand and try the other for some time, hence there was

"kinaesthesis in forefinger of other hand" when "muscles would go only as fast as the slower rate" (R U in xxiii C).

"Rigidity of forearm, extreme fatigue, and tension in left arm. Found my right arm in sympathetic rigidity and sympathetic tapping. General stiffness all round body." (L V in xviii A).

It is quite different, however, with subject R O who reports a sort of paralysis of will when he couldn't react and

"Instead of a definite specific movement, there was as a result a diffuse general movement over the body. Finally I jumped up from the chair."

The diffusion here was undoubtedly due to inhibition, and far from facilitating the proper movements, it all the more interfered with their rapid execution. For it stands to reason that while a certain amount of general excitability may increase the work-

power of a particular set of muscles, the general diffusion of a specific impulse over various channels not directly related to the muscles actually engaged must lessen the effectiveness of the latter.

One subject (S R) was observed to mark time with his right foot while striking the keys. This type of synkinesis seems to be a rhythm phenomenon and was performed unconsciously.

Cue of Movement.

With regard to the rôle of imagery in the initiation of movement and general guidance during the work, there are good reasons for supposing that nearly all, if not all, subjects resort to images of one sort or another, mainly kinaesthetic, while performing their finger movements.[104] In many instances kinaesthetic images and sensations were all the subjects were aware of at the moment the cylinder stopped revolving, and they were asked, in accordance with previous instructions for *momentary* introspection. In these momentary introspection tests only three subjects omitted to say anything about images, two of them doing the poorest work of all.

Some of the references to imagery recorded in the protocols follow below.

Subject F G

"Inner speech or kinaesthesis meaning 'right' 'left' " (xx A). "Kinaesthetic feeling of red with down [movement]. Fusion of green with kinaesthetic feeling 'up,' meaning 'all right' " (xix C aux.). "Perceiving the green dot and kinaesthetically perceived the movement for it and corresponding with red, and satisfaction with it" (xxi C aux.).) "Kinaesthetically made [initiated?] movement which meant red down" (xxiii C aux.).

Subject L L

"Towards end I had some false anticipations, sometimes I would be right, but many times wrong. I get it from memory. Partly visual and partly kinaesthetic in fingers and organic" (xix

[104] Cf. H. S. Langfeld: Voluntary Movement under Positive and Negative Instruction, vol. XX, 1913, p. 473 and also J. C. Barnes: Voluntary Isolation of Control in a Natural Muscle Group. Psych. Review, Monograph Supplements 93, pp. 42-43.

B). "Had kinaesthetic memory of the dots, but not correct all the time. Felt it in my fingers, so that I would not react all the time upon what I saw. This is not mechanical rhythm simply, because I felt what was coming. This stood in good stead when eyes grew fatigued. Had however no idea of numbers in this imagery" (xiv B). "Kinaesthetic imagery here too. . . . This imagery helped me to read the dots better" (xv B). "Tilting of head and vague inner speech goes with different impulses" (xix A).

Subject L O

"Saw red coming and was preparing for red *saying to myself* '2 red' " (xxi C aux.). "Tension in fingers and verbal affair in mind. Some joke in margin. . . . Have *visual-kinaesthetic* images of up and down" (xix C). "Verbally saying to myself what comments I should give when stopped. Internal speech always accompanies my work." (xxi B aux.) "Verbal commentary. Phrases like 'that's rhythmical,' 'that's pretty good' and so on." (xxiii C aux.)

Subject M L

"Feeling of tension of upward movement. Just about to strike for green, i.e., muscular sensation was in forefinger *meaning upward movement.*" (xxiii C aux.) "Tension in fingers and preparedness to go on when I was stopped" (xxi B aux.).

Subject R O

"Clue to the movement is kinaesthetic image of movement" (II, 15"). "With the tapping of the down, there was a coincident getting ready, a premonitory feeling of the next perception. While the green was in the periphery, the getting ready [for it] was in the organic background, but can't localize it" (xxiii C aux.).

Subject R U

"Counted internally 1 — 2 — 3 as you stopped me. . . . Remember the greens and reds in *motor terms of position* and as I tap them I count them. It seems as if I don't memorize the *color,* because I didn't say '3 red, 3 green,' but '3 of this' and '3 of that.' " (xxi B aux.) "Kinaesthetic sensation from the red I was tapping and perception of a green. Internal speech of 'green,' and something like a *kinaesthetic-visual* image of my finger going up, localized in right place. Hadn't yet reacted" xxi C aux.).

Subject V L

"Know that I have tapped correctly by auditory memory. Haste or trepidation will destroy the after-image of the count, and the consciousness or feeling of pressure" (xxiii C). "There were three sets of elements: (a) visual of red and of green, (b) multiplication—auditory imagery, also verbal and kinaesthetic, (c) association of red with forefinger, association of green with middle finger. Now in consciousness any one of these six classes might be prominent."

The reference here is evidently to the foreperiod prior to making the movement.

Subject L Z stated merely that red *meant* to him "down," while green meant "up." He told, however, of having internal speech (saying to myself '2 reds' "), but did not speak of any other imagery.

RECOVERY.

Imagery is also serviceable in the final readjustment after inhibition.

"To get right again, I must recall the foreperiod again" (F G in xviii A). "When starting anew, inner speech would reinforce coordination" (R U in xvi A). "In the blind tapping there is at least this central: that there is a tendency to break away from it: I recognize that I am wrong. It may be a verbal statement or auditory imagery (L O in xxi A).

Inner speech is sometimes resorted to as a protective measure to ward off an imminent break. Thus:

"I felt that my control was not equal to the task. I saw the dots, but couldn't hit them." Inner speech came in then" (R U in xvii A). "Inner speech . . . most pronounced when the dots are thickest, [i.e.,] where there is greater speed required. Say '3 red, 2 green, 3 red' etc." (LZ in xxi A).

There are other accounts of recovery in which imagery is not explicitely reported, but where the presence of some imaginal content may well be suspected.

"After the motor breakdown," says subject L L, "there is a period when I can't associate the dots with the movement. But gradually the association comes back again." (xxi A).

But how? He says further that there is some

"feeling connected with the building-up—a feeling that it's all right."

But this feeling need not be devoid of organic and kinaesthetic elements in general. There is the same inadequacy in the description of subject L E, who says that after an associative stupor

"I feel the gradual building-up of association" (xviii A). At the same time it may be assumed that the associative recovery and the motor recovery are not simultaneous, that the former is prior to the latter, and that imagery might go with the one and not with the other, or at least that it might be more difficult to detect in the one than in the other.

Momentary Introspection.

The momentary introspection tests which were adopted as auxiliary means to facilitate analysis revealed some interesting facts. Some of the results have already been cited under separate headings, but it would be tedious to have an exhaustive index of all the details that were observed by the subjects during the working period. Generally the momentary introspection test brought out the mental content of the moment only at which the work was abruptly stopped. In some cases, however, it would seem as if the very abruptness of the stop served to stimulate introspection. The introspective account does not even here necessarily cover the whole working period, but may be confined to a single moment or two. That moment, however, is closely analyzed and is typical of a great many moments of the working period. A few miscellaneous quotations from the protocols will show the value of such a method.

"I had just made a downstroke. I had a general idea of reaction, but not of a specific reaction. Consciousness that I had reacted to a red, and started on some reaction, but it was only after you stopped me that I saw it was green. You stopped me just as I ended the red" (R O in xxi C). "Feeling of tension of upward movement. Whether I tapped or just about to, I don't know. *Impulse* = perception of dots + general feeling of tension. The general feeling of tension has discriminated parts, sometimes meaning red and sometimes green. No instruction before me in words, but a general attitude. In rapid tapping, attitude becomes more tense, rigidity in back. When things go

wrong, the attitude is the same, ***but in going more wrong there is a tendency to relax and the instruction which is the attitude changes.***" (M L in xxiii C) "'I tapped a green just when I should have tapped a red. I tapped 3 greens and observed red, but then greens came up and was going to tap the red [that just passed], but the greens took up my impulse. A little annoyed" (F G in xxiii C).

The occasional inability to stop immediately after the signal is observed is something that impressed the subjects considerably.

"You got me in down flight, and even after you stopped me, I made a down movement. 'Go ahead' in consciousness" (R O). "Saw red and green after [apparatus] stopped, but couldn't stop immediately. Finished it up" (L E).
Subject L Z said once he stopped immediately when his finger was seen fluctuating for some seconds after the signal was given. At another time, however, he said his finger was already starting to move after he was supposed to have stopped.

It is evident that it takes a distinct effort to stop at a given moment, just as it takes an effort to switch over from one key to the other in accordance with the impulse to do so.

There is a certain amount of inertia to overcome in both cases.

CHAPTER VIII.

Individual Differences.

I

That there would be marked individual differences in the mode of reaction to the whole situation can easily be expected from an experiment such as the one under discussion. The movements in the response were simple, and yet, judging from the results it is evident that a number of possibilities are open even in a simple task—simple but difficult, on account of the time consideration. There is scarcely any doubt that this latter element was instrumental in bringing to light many individual differences that would otherwise have remained latent, just as in actual life, it is under conditions of strain that a person's character gets to be known.

In general, most of the subjects fall into two distinct groups in respect to several tendencies, and there is reason to believe that these various tendencies are not totally disconnected. An indeterminate or intermediate group is made up of those who do not show any pronounced leanings in their methods or who vacillate between two extremes. The subjects included under this group are few as compared with the others; and as a rule, even the most indeterminate or most inconsistent behavior bears the earmarks of some sort of affiliation with the one group rather than with the other.

1. The most elementary division is of course that of (a) *quick* and (b) *slow* reagents. All of the quick reagents had had some practice with musical instruments or in typewriting, but since one or two among the slow reagents had also had training of this sort, we must conclude that finger practice is not the sole determining factor of the reagent's speed. Most probably other variables entered in as well—these depending on the individuality of the subject. Those of medium speed may be grouped apart.[106]

2. The next most salient divergence, and one less obvious, is that between (a) the confusers and (b) the omitters. The former seem to lay most stress on speed. Their omissions do not extend over large stretches, but represent solitary spots here and there, while the latter, when hard-pressed, stop working for 10, 15 or even 20 seconds. They have this advantage, however, that they very rarely make a reversal and never carry on any random tapping. Confusion for them is immediately followed by a complete break in motor activity. The confusers, on the other hand, do not stop until they regain their full associative clearness, but keep on reacting incorrectly for some time.

It is noteworthy that the *omitting* group is made up of the slowest reagents, while those who do the rapid work are given

105 STERN's moderate view and broad definition of "psychological type" would obviate such objections as THORNDIKE's. cf. STERN: Differentielle Psychologie, chap. XII.

106 Cf. O. OEHRN: Experimentelle Untersuchungen zur Individualpsychologie, Kraepelin's Arbeiten, vol. I, p. 146.

to confusing. The one who seemed to disregard the stimuli most was the one who did the most rapid work. In many records he actually made more movements than the stimuli called for, but a considerable percentage of these movements were wrong reactions. The evidence, in general, seems to point to a certain correlation between quickness and the tendency to confuse, on the one hand, and between slowness and motor inhibitability, on the other.[107]

The question naturally arises as to the grounds upon which we judge a subject to be slow or quick. Is it on the basis of the actual number of movements? Then is it not a rather circular procedure to say that the omitters are the slow reagents? For had they chosen to make mistakes rather than to omit when confused, they would likely be classed with the quick reagents.

To be frank, the writer sees no way out of it, as the different tests that were given the subjects do not reveal any clear-cut index of their quickness to react in a given situation. In the 30 second tapping test, some of the slowest subjects manage to make a high mark, while in the "spontaneous record" test, the slowest subject takes the second rank as to the number of reactions performed. All this goes to show that tapping ability is quite a different thing from the ability to react quickly under interference conditions; and when a subject is here spoken of as being slow or quick, the reference is to his *speed under pressure and against time.*

The writer does not wish to convey the perfectly obvious idea that those subjects who are given to omitting will be regarded as slow reagents, but rather that those who are slow to react upon a situation such as we are dealing with are the ones to be affected by interference conditions in such a way as to have long inhibi-

[107] Several attempts have been made to correlate personality with the speed and energy of one's movements. That of PEREZ is probably the most noteworthy. His belief was that personality depended upon the degree of quickness or slowness combined with the degree of energy an individual is endowed with. (B. Perez: Le Caractère et les Mouvements, Rev. Philos., XXXI, 1891, p. 50.

For a classification of temperaments based on the subject's celerity of movement, cf. also W. W. DAVIS: Researches in Cross Education, Studies Yale Psych. Lab. VIII, pp. 85 ff.

tions during which their activity is completely paralyzed. The two propositions are certainly not identical in significance. The point does not hinge on the *degree of omission* in either of the two groups, but on the fact that we are dealing with two different types of people characterized by *two different tendencies.*

3. (a) *Equalization* and (b) *undulation* form another set of differences, i.e., some subjects show a tendency to attempt every group of stimuli, while the reaction method of others presents crests and nodes. One group will be tapped out completely, while another group is allowed to pass without the least response to it. The subjects who follow the first method tend to equalize the groups. Their efforts are more distributed, while those who are inclined to complete a whole group before they pass on to another, show a tendency to favor the larger groups and omit isolated dots. Here too there are signs of correlation, the omitters corresponding to the undulators; the confusers to the equalizers.

4. As to the manipulation of the groups, it has already been stated earlier that the method varies not only with the subject, but with the situation. The rule is to group the dots, but several subjects, at times, reacted to the dots individually, relapsing to the grouping device whenever they found themselves hard-pressed for time. One subject was inclined to react to the dots individually most of the time, while two others not only grouped the dots, but, at the high speed, grouped the groups.

5. An interesting individual difference was brought out by the way in which preference was shown for either the ascending rate or descending rate of speed. Some—and these were the quick reagents—liked to begin with the most difficult end of the list first so that they could relax afterwards. Others, representing the slow reagents, preferred and did much better when the arrangement of the stimuli called for a gradual speeding-up. That this division is, in the last analysis, not due to actual individual differences, but is dependent on the relative difficulty of the rate, to begin with, will be explained in the section on applications to education and pedagogy.

6. There is still to note that the gradual change of speed does

not affect all subjects in the same way. With the majority th is a tendency to speed up and to slow down *pari passu* with rate. Subject LV who is the quickest reagent finds it diffi to slow down after starting out at full speed. The slow re ents, on the other hand, hardly ever speed up. Two of th TR and TT, keep a steady pace or nearly so all through record, while two others, SR and ZB, actually do less work the high speed than at the slow rate. This falling off, in th case, is due to the long inhibitions that come over them, dur which their activity is brought to a standstill. It may be ad too that the quick reagents find it more difficult to stop the n mentum of a given impulse than the slow reagents; also t single reactions occur much more frequently in the records the former, while the latter follow more closely the instruct to react twice for every stimulus.

B. Individual Characteristics.

The differences that have just been discussed are really gr differences rather than individual differences, though usu spoken of as such. To be sure, we must not assume that ev single subject will display entirely different tendencies. A ma festation of individuality to that extent would be too much expect at present even from the minutest study of individ differences. For the time being, it is sufficient to classify a b of subjects according to type, with the understanding that wit a given group, the subjects will differ amongst themselves *degree.*

But this in itself already constitutes a basis for further alysis. Moreover, the *particular combination* of tenden serves to single out the individual from amongst his group; in this way we can make, at least, a superficial examination every subject individually. The individual remarks to foll are an epitome of the series of observations recorded by the perimenter as a supplement to the computations and introspect protocols.

Remarks.

Subject FG

Quick reagent. Uses kinaesthetic imagery almost exclusively. Given to reversals. Takes task most coolly. Reacts with attention in the margin. Least inhibition. Shows greatest tendency towards compensation as well as lapsing into single reactions (instead of double ones). Attempts every group. Preference for easier movement less decided than with other subjects. Shows evidence of rhythm by *recurrence* of same mistakes in succeeding revolutions.

Subject GD

Medium speed. Imagery, kinaesthetic and visual, confuses somewhat, and experiences a great deal of sensory inhibition. Compensation marked. Breaks down at about the same spot in each revolution. Repetition of former errors. Work regular and tendency towards rhythm. Thus if subject once starts on an odd number of reactions, he will keep on tapping an odd number for some time. Lays emphasis on group, but changes his method to suit conditions.

Subject LE

Medium speed. Kinaesthetic Imagery. Given to compensation. Tendency to make up in slow part for loss at high speed. Favors rhythmic movements. Tendency to make an odd number of reactions. Frequently makes superfluous reactions. Numerous reversals and confusion.

Subject LL

Somewhat slow reagent. Mixed type of imagery with voco-motor and organic elements prominent. Synkinesis and compensation much in evidence. Movements irregular. Picks up towards the end of the work. Leaves the last part of the one revolution so that he could start well on the next revolution. Shows signs of "nervousness" and fumbling in his movements. Affective tone pronounced.

Subject LO

Speed medium. Visual and voco-motor imagery and some

auditory also. Does uniform work, but affected greatly by inhibition. Blind tapping, yet manages to maintain a certain order (rhythmic?) during such reactions. Slurs the single stimuli for the sake of the groups, yet does not complete the whole group. Noticeable compensation and some transposition.

Subject LV

Quickest reagent as to output of work. Auditory imagery predominant, also voco-motor and kinaesthetic. Gets after-images of green stimuli (=more difficult movement). A great deal of confusion and blind tapping. ***Far greater innervation than necessary***, especially at high speed, with the result that there were more superfluous movements than omissions. Emphasizes odd number, tapping three times instead of twice for one stimulus, 5 times for 2 stimuli, and 7 times for 3 stimuli. Work uneven, strokes on kymograph at various distances apart. Repeats former mistakes. Much compensation and synkinesis. Most of confusion occurs with the more difficult. With practice, the innervation becomes less exaggerated and more normal, insuring greater accuracy, but causing more omissions.

Subject LZ

Slowest reagent. Visual type. Some voco-motor imagery, but no kinaesthesis otherwise. Simply omits, but does not confuse. Scarcely any single reactions. Sticks entirely to the instructions asking for a double reaction. Additional taps very few. The group is for him important; hence he favors the larger groups and those he reacts to he completes. Long inhibitions coming on periodically. Compensation, transposition, and other tendencies of a spontaneous or mechanical sort ***not found***. Gains a lot by practice.

Subject ML

Most indeterminate subject. Oscillates between quick and slow type. Imagery visual-kinaesthetic with organic elements. Changes his method frequently. At times, is given to making reversals; at times, he simply omits, but does not allow himself to be confused. Consciousness centred around more difficult movement. Compensation tendency slight. Strokes on record

point to regular work. Lapses frequently into single reactions and occasionally resorts to blind tapping.

Subject RO

Quick reagent. Kinaesthetic imagery. Energy or innervation increases and decreases gradually with rate of stimuli. Lapses into single reactions when rushed. Tendency to make up in slow period for the loss during high speed. As a rule, attempts every group. Determination and effort shown by motor diffusion throughout body. After very strenuous work shows a sudden breakdown. Then after a period of omission, he begins well afresh. Centres attention around the stimulus corresponding to the more difficult movement. Lets the other take care of itself. Does very regular work. Compensation tendency very slight.

Subject RU

Quick reagent: voco-motor, kinaesthetic, and visual imagery. Combination of automatisms—(a) must struggle against tendency to continue making the same movement. During slow part, spontaneously adds reactions while waiting for next group of stimuli. (b) Breakdowns seem to be periodic—one long one at the middle of the revolution and a shorter one towards the end. (c) Occasional rhythmic stretches, e.g., 2a, 2b, 2a instead of 2a, 1b, 3a. (d) Repeats mistakes of former revolutions, and sometimes seems to mechanize a certain arrangement regardless of the actual stimuli. In xxii B an additional single tap is found at the same place in 4 different revolutions. Compensation tendency slight, but presents a few cases of transposition. Of the quick reagents RU is the one least given to lapsing into single reactions. Confusion comes in with the more difficult movement, only very rarely with the easier one. Tends more than the others to react from memory, i.e., tries to catch up. Does not attempt every group as the other quick reagents, and slurs an individual dot for the sake of completing a group.

Subject SR

Next slowest reagent. Never reports kinaesthesis and is probably of the visual and auditory type. Does not speed up with

an increase in the rate, but keeps a steady pace. Very rarel allows himself to get confused, but simply omits at the hig speed. Reacts in pairs, as a rule. Compensations few. React in snatches, i.e., completes one group, then misses the next. Ha very long inhibitions. Does less work when the stimuli ar thickest.

Subject TR

Slow reagent. Visual and voco-motor imagery. Does not re port kinaesthesis. Seems influenced by negative attitude or feel ing that the task should not have been given because too diffi cult and exacting. Reacts with a certain regularity and de liberateness. Very few *mistakes* made, but a great many *omis sions*. Seldom makes a single reaction. Emphasizes the large group and slurs the isolated dot. Keeps a steady pace. N compensation. In general the work bears a striking resemblanc to that of LZ and SR.

Subject TT

Slow reagent, but on the way towards rapid improvemen Strokes on record show uneven work. Some incoordination ap parent. Much compensation and some confusion and blind tap ping. Adds in the slow part and magnifies the larger group, bu loses a lot at the high speed. (Data insufficient as to other char acteristics.)

II. PERSONAL DETERMINANTS AND PERSONALITY.

We have seen that the subjects fall into two types (with a intermediate, in one case an indeterminate, class) and that cer tain characteristics, at least certain constellations of tendencie are ascribable to every subject. It now behooves us to inquir into the significance of it all. In what direction are we to loo for an explanation of these individual differences that seem t have such a conspicuous place,[108] especially in the departmen STERN[109] refers to as Psychical Energetic? Most of us woul have thought that the adoption of a method for the workin

[108] Psychologische Arbeiten (KRAEPELIN) generally and particularl OEHRN's Exp. Studien zur Individualpsychologie in vol. I.

[109] W. STERN: Ueber Psychologie der Individuellen Differenzen, p. 117.

out of a problem is inseparably bound up with the mental make-up of the individual subject, that it is not a merely accidental affair. It is disappointing then to read in WASHBURN's thoughtful treatise on movement an utterance which, far from shedding any light on the subject, only serves to befog the issue. In attempting to account for the different stereotyped methods that subjects adopt when given vague and indefinite instructions, she says:

"It seems to me that these self-imposed problems can be explained simply in the following way. A very general problem sets in readiness a number of methods for its solution. One of these methods happens to be the first one adopted, and simply perseverates: since the influence of perseveration involves so much less fatigue than the influence of the activity attitude, it is not interfered with and becomes increasingly strong."[110]

We must all admit it a laudable undertaking to reduce an explanation to the lowest terms, but when these terms read zero for both the numerator and the denominator, we feel that the simplification has been carried a bit too far. To account for individual differences in the performance of a task by saying that they arose accidentally is merely to take refuge in an asylum of ignorance. In former times, it was the *miracle* that was appealed to as an all-embracing explanation. More recently, it would seem that *chance* is occupying this position of a wholesale difficulty-resolver.

In the writer's opinion to dispose of the matter in this way is no more satisfactory than the child's replying "because" to questions it cannot or would not answer. If we examine WASHBURN's tentative explanation, we shall see that it rests on half a dozen hypotheses, one propped up by another.

Suppose, for a moment, we applied the theory in our particular case. Shall we say that some subjects *happened* to lay more stress on completing a group or on making a double reaction or on the fact that they are not to make mistakes, while others just *happened* to emphasize other factors? It is needless here to analyze the concept of chance and to distinguish between the

[110] M. WASHBURN Movement and Mental Imagery, p. 173

strict meaning of the word and its conversational content,
this has been done so often. Suffice it to say only that the w
"happen" as applied to a person's action is different from
connotation it has when employed in connection with the thr
ing of dice. "Oh, I just happened to be thinking about it, th
all" we dispose of our curious friend's query. Yet any
who has had a smattering of psychology knows that we di
not happen to do it, in the strict sense of the word.

In the first place we might ask *how it happened* that a gi
subject should happen to hit on a certain method of doing thi
and not on another first. But secondly, WASHBURN has stil
prove that a method once hit upon will perseverate. Our res
certainly do not show it, and what is more, the facts flatly c
tradict this assumption. We have already seen that (a) m
ods are changed to suit the circumstances, (b) subject ML
quently vacillates between different methods. In addition, h
ever, the possible correlation of tendencies among the diffe
groups seems to indicate that we are dealing here with a m
complex arrangement than might be supposed from WA
BURN's account.

Were it not for the fact that there is a growing tendency
certain quarters[111] to simplify matters by disregarding them
shifting the emphasis on some irrelevant point, it would ha
be worth while to discuss WASHBURN's view at such great len
She herself probably recognizes the innate character of a g
many individual differences, at least, but if so, it is all the m
astonishing that individual differences of such a marked
significant character as those falling under the class of *energ*
should be treated so unceremoniously. What prevents us, on t
score, from maintaining that differences in imagination are

[111] Aesthetic preferences are sometimes explained after this naïve fas
or by attributing them invariably to childhood experiences. Barring
limited number of cases of dislike to which FREUD's repression theory m
apply, this hypothesis seems to be a vagary which brings to light the def
ive reaction of the less fortunate people in matters of taste. They migh
interpreted as saying "Our aesthetic appreciation, it is true, is not wha
ought to be, but then we have met with an accident, and are not to bl
for it."

only to fortuitous variation, that we *happened* to hit upon a certain type of imagery in early infancy and—after applying the rest of the hierarchy of hypotheses—here we are, using one type of imagery more than another?[112]

The whole point of view that is attached here betrays a hankering after an obsolescent, if not obsolete, *Weltanschauung*, viz., that of historical materialism, but without its philosophical breadth; for BUCKLE only denied the existence of original traits, but it never occurred to him to explain palpable differences on the basis of fortuitous events. The *tabula rasa* doctrine, in its extreme form, or better still, the *tabula rasa temperament* is at the root of this whole "scientific" method.

If individual differences stood for nothing more than chance happening developed by habit, differential psychology would not have made such phenomenal progress as it did within the last two decades; and it is to be hoped that in the future there will be more and more intercourse between that branch of psychology and physiological psychology, and also biology. For the history of a person's individual tendencies—even as regards interpreting and acting out indefinite instructions or adopting a particular method as distinct from others—begins not only with the protoplasm of a given individual, but farther back.

At the present stage, a task of such proportions, i.e., of tracing the origins of individual differences, would naturally be out of the question. But surely we have a right to postulate that in the individual psychophysical make-up, there is a *something* that determines him to take one course of action rather than another;[113] and inasmuch as this *something* manifests itself in innumerable ways, and varies from individual to individual, we

[112] It is of course realized that WATSON, and happily not more than one or two others, would have us first prove that there is such a thing as imagery altogether, but until they prove that there is such a thing as sensation, or else adopt an objective terminology like that of NUEL, we are quite e; and if they some day decide upon the latter course, we are even ... er. . . .

[113] *Cf.* JUDD, (Movement and Consciousness, Psych. Review Monograph Supplements, vol. VII, p. 215). "Individual nervous organizations are clearly of importance, as shown by the fact that certain types of reactions are repeated by given individuals."

must recognize that we are dealing with individual determi tendencies. Again when we ask ourselves what further si cance these individual tendencies have, the reply will be th is these tendencies that constitute the *personality* of the ind ual. Hence we might as well substitute the term "persona terminants" for that of individual determining tendencies is by a person's acts that we judge his personality, and wh valid or not, we have to accept for practical purposes the saying that a "person is what he does."

For a long time character has been associated with volit acts.* Ethics was largely instrumental in throwing this co tion into relief, but scientifically nothing had been done to p the relation. The first experimental attempt to study pers ity in the laboratory was made by ACH[114] who evidently tho the will-act peculiarly suitable for bringing to light tem mental differences. Yet the brief space in his book devote observations on temperament by no means justifies the tentious title of the book, even if we include the scattered erences in the body of the work to that phase of his sub The part on temperament is nothing more than a side-issue him.

All that ACH's experiments really amount to in their rel to temperament is to show how certain phenomena that he observed in connection with will activity do fit into the scheme of temperament. He did not start the subject anew had the ancient classification to begin with, and he m grouped his subjects in the one category or the other, on basis of their determination, motivation and excitability. only new and important fact that he contributes is the add of a fifth type of temperament to the traditional set of which he calls the *deliberate* (*besonnene*) type. It is marke a tendency to obviate all the obstacles that interfere wi

* Cf. Ed. Hirt: Ueber empirisch begründete Bewertung der normale pathologischen Handschrift. Archiv für die gesamte Psychologie, vol. X 1918, p. 358. "An Stelle des allgemeinen Begriffs des Willenstypus darum heute die einzelne Willensbewegung zum individualpsychologi Problem."

[114] Ach: Ueber das Denken und das Temperament

ıpt reaction, and to avoid slips. The affective reaction be-
c ıes less intense as the determination towards an accurate and
ı l reaction becomes stronger. With these subjects, success
accompanied by the awareness (*Bewusstheit*) of "I am able," ıile failure brought on a state of self-irony with the aware- "If I only seriously want to." Great associability of ideas ɜo a mark of this temperament.

It is doubtful as to how much we can build on such a tenuous basis. In a study of personality we cannot be satisfied with the observation of one or two traits and these gained mainly from introspection. We want to get at all the personal determinants possible by testing the individual not only as to determination, but as to every mode of reaction we can think of. Even the sort of imagery one uses is a valuable partial index, as MARTIN has shown in her recent address.[115] It is only then that the facts could be collated, and undoubtedly some correlation will come to light. It is this very correlation that we are seeking. For as THORNDIKE remarks

"A statement of the differences between one whole man and another would be an almost interminable inventory of particular differences, unless some traits were so related that knowledge of the amount of one of them possessed by a man informed us of the amount he possessed of the other also"[116]

Why should we expect to find correlation between traits and tendencies? The answer is: common observation puts us on this track. The very classification of people according to the four types of temperament bears evidence of a universal feeling, from the time of antiquity, that a given personality takes a more or less definite direction. This view stood the test of the critical philosophy, for KANT appears to have held the ancient classification in a modified form,[117] and WUNDT adopts a similar scheme. We may question the adequacy of the ancient classification or its

[115] Lillien J. Martin: Personality as Revealed by the Content of Images. (Address of the retiring vice-president before the meeting of the American Psychological Association 1916).

[116] THORNDIKE: Educational Psychology, vol. III, p. 348.

[117] WUNDT: Grundzuege der Physiologischen Psychologie vol. III, p. 637 5th ed.

amended form, but the possibility of some sort of classific cannot be denied. In the first place, it is a practical nece We are constantly judging our fellow-men as individuals out taking into account all their traits. Perhaps five minut conversation will be sufficient to give us an impression. we are in need of is the standardization of these judgm But secondly, it stands to reason that a number of traits n have a common factor at their root. Thus a person who slowly and deliberately is apt to differ from the quick indiv in everything involving their constitutional differences tha responsible for the divergence in the rate of their spea This would presuppose a limited number of personal deter ants, and biologically, a limited number of unit characters innumerable permutations and combinations.

THORNDIKE, in his eagerness to show men nearly equal a give the vast majority of them a modicum of everything so they may all be mediocre, fails to distinguish in his treatme individual differences between intelligence traits and person (character and temperament) traits, with the result that thinking in terms of the former and is applying his conclu promiscuously.

Thus, we can follow him when he says

> "Mental traits are certainly not as a rule unit characters, of two or three cooperating unit characters. On the cont most of them seem to be the results of very many unit c acters."[118]

only if he is referring to an intelligence trait. Certainly a like the rate of learning, for instance, is dependent upon many factors, but when we turn our attention to temperam traits, the situation is different. Here, on the contrary, should be inclined to reduce the number of unit characters cause several tendencies might easily owe their origin to same unit character.

Individual differences have hitherto been treated as the appendages of an experiment. They formed a side-issue, no more. Even when two traits, say two abilities, were b

[118] THORNDIKE, loc. cit., p. 268.

correlated, the emphasis would be not on the individual, but on the abilities that were being examined. Most investigations take up the question of individual differences arising out of the experiment. They simply cannot be ignored now-a-days, but in very few cases do these individual differences appear as more than *disjecta membra*. That is especially true of temperamental differences. If only a suggestion along these lines, made in one investigation, were followed up in other and subsequent investigations, a large body of data would accumulate that would point definitely in one direction or another. Gradually these suggestions would either develop into facts or else they would be thrown overboard.[119]

To illustrate: in 1873, EXNER was led to the conclusion that

"Man im allgemeinen bei solchen Individuen die kürzeste Reactionszeit zu finden erwarten darf, welche am meisten gewohnt sind, ihre Aufmerksamkeit auf einen Gegenstand zu concentriren, und dass solche Leute, welche ihre Vorstellungen ungehemmt ablaufen zu lassen gewohnt sind, grössere Reactionszeiten haben."[120]

In 1913, LANGFELD was led to the conclusion that those

"who trusted most to a non-represented determining tendency did the best" [work in passing a stylus down a groove]

saying further,

"This suggests why a certain type of mind is unskilled in movement, that is the imaginative type which is continually thinking of other associated things. As there are more of the imaginative among the intellectual, it is not surprising that there is more skill among the non-intellectual."[121]

Surely this is a sufficiently important point for consideration in reaction experiments of any kind.

Worse still is the lack of cooperation or communion among the members of the laboratory with the special purpose in view of studying and correlating individual differences. Instead of a

[119] Cf. W. STERN's reflections on the subject in his "Differentielle Psychologie," chap. VII. (Arbeitsgemeinschaft und Sammelforschung).

[120] Exner: Untersuchung d. Einfachsten Psychischen Prozesse. Archiv. für die Gesamte Physiologie (Pflueger), vol. VII, 1873, p. 614.

[121] Langfeld: Voluntary Movement under Positive and Negative Instruction. Psych. Review, vol XX 1913, p. 477

connected chain we get only a large number of links that never put together, because one experimenter does not know details of the investigations carried on by other experimen in the same laboratory.

There are only one or two other investigations accessibl the present writer in this respect, i.e., with references to a n ber of subjects that had also taken part in the research u discussion. In one by H. T. MOORE,[122] all the subjects he ployed, excepting the present writer, were also employed as jects in our own research, but seeing that the field is so re from the one we are concerned with, we can hardly expec find any possible connection between the results of the va individuals. And yet, it may seem curious, there is the that his very consonant subjects were our very slow subj while the two that appear in the dissonant group were am the very quickest reagents in our experiment; the most dis ant was actually the one who turned out the greatest numbe reactions, though a great many were wrong. Now all this be merely a coincidence, but there is no harm in taking not coincidences.

In the other research, that by H. E. BURTT,[123] the relatio much closer to our work; and we feel that it is no coincid when two of his subjects who seem most susceptible to dist tion are the slowest reagents, or rather, as has been already plained, are most affected by the interference conditions of experiment. More than that, the subject who "showed the e of the distraction most markedly" is the subject who would b by far the highest inhibition coefficient under conditions of in ference. Furthermore, in the imagery experiment he displa "fear of making a mistake"; in our experiment he shows least tendency to reverse the association and make mistakes way.

It so "happens" too that this subject is of a decidedly vi

[122] H. T. MOORE: The Genetic Aspect of Consonance and Disson Psychological Review, Monograph Supplements, vol. XVII, 1914.

[123] H. E. BURTT: Factors which Influence the Arousal of Primary Vi Image, American Journal of Psychology, vol. XXVII, 1916.

type, while the other who keeps company with him is, to all appearances, visual also, though not in so pronounced a way as the former. Both of these subjects never mentioned the occurrence of kinaesthetic imagery while engaged in our investigation.

From the foregoing it is possible to infer that the decidedly visual type is more susceptible to distraction, interference conditions and the like; and if we turn to another experimental report on an allied subject,[124] we shall find, as one of the results, that the most visual of three subjects who participated in that particular investigation was delayed most in sorting cards under the distraction of reading aloud.

Are we to regard all these relations as coincidences, merely due to chance? Or shall we take them up as beginnings for further study and analysis? From a methodological point of view there can hardly be two opinions on that matter; and anyone who shuts his eyes to such relations or correlations displays a certain narrowness that is, alas, rampant in experimental psychology and proves an obstacle in the way of proper interpretation of the facts. If we are to get all that is to be gotten out of our results, we must make them a stepping-stone for further research. They should have an arrowhead tacked on to them, so that we might be able to orient ourselves with respect to other investigations and problems in related fields. Hence the experimentalist must be possessed of a sense of direction.

A serious drawback in the study of individual differences and their correlation is the complexity of factors that we are confronted with. Many investigators probably feel as if they would never be able to disentangle the numerous threads that may be involved in a single result. Take the instances that we have cited—and here we have a relatively simple situation: How shall we explain, discarding the view that it is a coincidence, the relation between belonging to the visual type of imagery and being susceptible to distraction, or between either of these and slowness under interference conditions and all the other traits that go

[124] M. F. WASHBURN and M. McMEIN: The Effect of Mental Type on the Interference of Motor Habits. American Journal of Psychology, vol. XX, 1909, p. 284.

with it (complete inhibition, undulation, lack of reversals and mistakes, etc.)? Is the type of imagery directly responsible for these other modes of reaction, or is there some common factor giving rise to both the former and the latter, that is to say, the connection would have to be sought farther back in the nervous make-up of the individuals? To answer such questions at the present stage of our knowledge would be a premature venture. We must be patient in regard to this point just as in scores of others.

But of what advantage—it will be asked—is it then to talk of such indefinite terms as dominant determining tendencies and personal determinants? Why add to our already increasingly burdensome and unwieldy terminology? Our reply is that these terms, though *structurally* indefinite, are fraught with a very definite meaning functionally. Their value is of a *heuristic* kind. They, at least, pave the way for a solution in indicating what turn we are to take.

In employing such terms as determining tendencies and personal determinants, we are, like the mathematician, *dealing with unknown quantities.* Now X, Y, and Z, may stand for any quantity, to begin with; yet they have a definite relation in the equation, which works out correctly after proper arrangement of the various terms. Is this not precisely the method we are adopting in psychology? First we must have all our terms at our disposal. We then place them in their proper positions by bringing one fact into relation with another. A great many irrelevant data will be dropped, and probably a solid core will remain which it shall then be our task to reduce to a structural category or to translate into physiological language.

The reason why we have chosen to classify determining tendencies into three different classes should be quite plain. They were introduced because there was a real demand for them. We do not believe all determining tendencies equal because, in the first place, there seems to be a *leitmotif* operating universally, which largely shapes the course of the ordinary determining tendencies—those touched off by the experimenter's instructions. But further the fact that each subject has his own way of doing

things (difference of degree and combination, *vide supra*) proves that the determining tendencies set into play by the instructions are also modified by other tendencies, varying with the individual, which we, on that account, dubbed personal determinants.

Applying the above relation in the concrete, we have the following state of affairs taking place in our experiment. The subject, in accordance with the instructions, proceeds to depress the one key twice for the red stimulus and to depress the other key twice for the green stimulus. But the dominant tendency to take the course of least exertion will affect his work in such a way that he will emphasize the easier movement and slur the more difficult one, while his personal determinants will direct his mode of reaction in a more specific way,[125] which will vary from that of another individual. If X, Y and Z are represented as the dominant determining tendency, the determining tendencies, and the personal determinants, respectively, then we may express the relation of Y as a function of X and Z, or in symbolic form, $Y = \int (X, Z)$.

The term "physiological cortical set" has had a wide vogue of late in psychology. The concept is a useful one, and though it can scarcely be considered an adequate explanation of the determining tendency, it serves to bring us closer towards some tentative theory. If we accept the "cortical set" as the physiological equivalent of the determining tendency touched off by the instructions, we must remember to take our previous conclusion into consideration. This demands that we make the set dependent upon other prior arrangements which are the physiological equivalents of the dominant determining tendencies and personal determinants.

But why assume such a thing as personal determinants, in the first place? Can individual differences not be explained on the principle of different constellations of determining tendencies? No, that would be a circular procedure, because the determining tendencies do not operate until the instructions have been giver

[125] Cf. in this connection G. V. HAMILTON's "Specific Organic Properties" or reactive tendencies in Sexual Tendencies in Monkeys. Journal of Animal Behavior, IV, 1914, p. 314 ff., especially p. 31.

to the subject. Now, how is it that one particular conste should form and not another, unless we suppose that in th vidual there is a something which determines that formatic not another?

We fully admit that the working of these different tend can, at most, be only a matter of conjecture at present; l any rate, it is far better to be in the trial stage than to the problem entirely, by attributing a given mode of react mere chance. In the former case we can at least hope to solution some day; in the latter, we cannot so much as n start.

CHAPTER IX.

Applications.

A. Results as Applied to Pedagogy and Education.

Although our problem is predominantly of theoretical significance, it would be worth while to draw a few inferences from the results with regard to such an important subject as education.

Naturally these inferences cannot be amplified so as to be brought into relation with the more typically educational topics such as memory, association and imagination. But for that very reason, i.e., just because these topics have been discussed over and over again, it would be proper to turn our attention to some more neglected points perhaps that suggest themselves as corollaries from our experimental findings.

In the first place, there is one result that is of special interest which was merely alluded to in the chapter on objective results. It was brought out incidentally and has no direct bearing upon the conflict of impulses. We can now discuss it at length because in connection with education it is quite in place.

Upon presenting the stimuli-lists both in an ascending and a descending rate of speed, varying the order in alternate weeks, it was observed that the subjects were almost equally divided as to their preference. Some would much rather have the close dots first and end up with those farther apart, while others preferred it the other way i.e., they wanted to begin with the farther apart dots first and gradually work up to the thicker part of the list. Not only were they decided about their preference, but the very question as to their preference seemed to them unnecessary. The former thought it of greater advantage to *everybody* to start with a dash and then slowly relax, while of the latter, two or three took the trouble of explaining in their introspection that it was the most natural way of doing things to start slowly and gradually keep increasing the speed. The objective results

all confirmed the subjective preferences. It may be said that only two subjects were not consistent in their preferences at the time. They seemed to be on the borderland and were subject probably to other variables that did not make so much difference with the rest.

To begin with, the *prima facie* fact of this division would clearly indicate the existence of individual differences with regard to performing a task. Some prefer to take a quick plunge into their work and enjoy the relief that is theirs, as the demand on them becomes diminishingly small; and it is in that way that they can do a better job. Others, however—and probably these are in the majority—must work their way up. They grow with their task.

Now all this is very true, and if we take the trouble to look around we shall find these two types everywhere. But that isn't where the matter ends. It was discovered later that the more difficult the rate was made by increasing the speed, the less prone were any of the subjects to prefer the rapid-slow lists. Not only was such a preference not marked by the previous consistency on the part of three subjects, but in some cases the latter had still stuck to their old statements about their preference while the objective results indicated that better work had been done in the slow-rapid list, whether it came first or second in the sitting. Also those who had previously been borderland cases, were now veering around to join the slow-rapid preference group.

As this observation was only a side-issue, it was not tested out thoroughly by reducing the rate for the other subjects who had preferred the slow-rapid lists from the very start. Nevertheless considering that those who made up the rapid-slow group were the quickest reagents, while the group preferring the opposite order of rates was composed of the slowest reagents, with the originally inconsistent group coming in between, we appear to have a clue to the whole situation; and it seems we can safely venture to offer the following formulation: When the task is difficult, to begin with, and becomes increasingly difficult, it is best to work up one's way gradually in order to obtain the best

results. On the other hand, if a task is easy, to begin with, and is steadily becoming easier, the best way to go about it is to tackle the more difficult end first and then gradually relax.

It is true that not all tasks allow of such treatment, but we can conceive of a great many that do and besides the principle may apply to a whole day's work in which a number of tasks are included.

It applies equally well in the matter of education. First, in regard to individual education, each one knows what subject or phase of subject is for him easier as compared with another subject. If he has, let us say, four or five subjects to review among which he finds two easy and two difficult, he ought to begin with the latter. But suppose he has a different proposition before him—this time not a review, but entirely new and difficult work, it would then be of advantage if he could start with something easier and switch off to the more exacting job later. Naturally the question of fatigue enters in here, and it is not necessary that he reserve such work to the very end, when he is worn out. This is, however, a matter to be judged and decided upon by the individual himself in accordance with the special conditions and circumstances that may be involved. The main point is that he is to avoid plunging into the very thick of the difficulty until he has had time to adjust and readjust himself, otherwise his impetus will receive a rebuff that will only impair his efficiency for the rest of the period, so that it does not increase *pari passu* with the growing facility of his task.

Second, as regards the planning and arranging of the school curriculum, we have in the foregoing rule a good guide as to what subjects the session should begin with. Much would, of course, depend upon the grade and age of the children, the period of the year and other such factors. We realize also that the school curriculum is not something that is amenable to frequent changes in conformity with varying conditions. There is also the consideration to reckon with that some children are more adapted to one thing than others, and that it would not be feasible to divide a class on the basis of such inclinations or preferences. Yet, for all that, though we may be doubtful as to

whether the study of a foreign language is more difficult than mathematics, or the reverse, there is no denying that certain subjects make a universal appeal to the pupils and call for less effort on their part. All things considered, it seems reasonable that, let us say, a history lesson would be more appropriate to open the session with than an exercise in Latin composition or an explanation of the binomial theorem. When in doubt as to the *relative* degree of ease and difficulty of the various subjects, so as to allow the application of our previously formulated principle, we should be inclined to think that it is safer to follow the easy-difficult instead of the difficult-easy path. As a matter of fact in many schools, a method such as this is adopted in the lower grades where singing or drawing is the first subject of the curriculum, but so far as the writer knows, no evidence has been presented to prove that such a plan is either justified or is only arbitrary.

These suggestions, it must be remembered, are put forward merely in a tentative way. The question of the order of ease and difficulty is, it seems, important enough, and yet it has been entirely ignored so far. In our present work, it forms only an incidental adjunct. It can scarcely be doubted that an investigation, begun along this line, would yield some clear-cut results.

2. In line with the preceding observation concerning the easy-difficult and difficult-easy preferences is the result, both introspective and objective, that after an inhibitory break in the reactions, the situation is, as a rule, regained by starting on the easier of the two movements, and the subject actually waits until a stimulus comes up which represents that movement. The conclusions that may be drawn from this are almost too obvious to require any expatiation. The class review before embarking on a new line of thought, the summary of a chapter before starting on a new topic strikes us as putting into effect a variation of the above result. With the teacher and the writer, the stepping-stone is a precautionary measure in order to avoid a break and confusion; in our work it served as a first aid after the accident has happened. Those teachers who allow their pupils to flounder in their inability to grapple with an elaborate and complex line

of thought rather than take one or two steps backwards to make the transition clear have evidently no insight into the details of their profession.

3. The emphasis that is here laid on gradual adaptation should not be extended too widely; for as we have seen our principle works in both directions and needs careful adjustment which is more practicable in the case of self-application than in applying it to others, especially when these are young children. By no means are the suggestions contained here to be interpreted as calling for an extremely easy plan of study or the setting of very slight tasks. Against this we are warned by another result which shows that if you make a task too easy, mistakes will occur just where they are least expected, thus proving that the loss of the feeling of responsibility and the particular set that goes with it is disproportionate to the degree of ease. A certain modicum of difficulty should attend every task.[126] The ways in which this object can be attained are many and will be ascertainable only after examining the particular situation. The difficulty may be increased sometimes by shortening the period within which the task must be completed, or by introducing some additional work, or even by surrounding the task with a halo of gravity, first as to the *idea involved,* second as to the *consequences entailed* in the event of failure, i.e., the method employed might be that of *exhortation* which would appeal to the better type of mind, and *warning* to those who must have other motives to guide them.

It seems somewhat strange, at the first flush, to think that ease or difficulty could be increased or reduced by such a seemingly artificial method, but the reason for such an attitude would be the sore want of analysis of the terms "easy" and "difficult." So far as the writer is aware no attempt had ever been made to distinguish between subjective difficulty and objective difficulty. The confusion that this results in can be seen from the fact that in English and French investigations, more stress is laid on the objective connotation of the terms, while the Germans often convert our easy and difficult into "*bequem*" and "*unbequem.*"

[126] Cf. COLVIN: The Learning Process, pp. 82-202.

In reality, as the term is employed ordinarily, many elem contribute to making a thing difficult, the *consciousness of difficulty* not being one of the least. There is an old story wl goes back to the "Once upon a time age" that proves how m insight some possibly untutored mind displayed in that con tion.

The story relates of a king who had proclaimed througl his kingdom that he wanted a tiny hole bored through sev very small diamonds in his possession, and that he who w be successful would rise to the height of glory, but any one undertook this piece of work and happened to break the diam would be punished by decapitation. A few of the most ex diamond cutters in that land risked their necks with fatal sults to themselves. Then one diamond cutter conceived a ha idea. He daringly asked for the last diamond and commissic one of his apprentices to perform the momentous operation, ing him that he must be very careful, as it was a costly af The apprentice accomplished the task successfully. The m that the story was calculated to bring out is obviously that high-strung attitude of the master diamond-cutters made it possible for them to achieve their object. For them the task came too difficult with the grave condition placed on it. For apprentice, however, the degree of difficulty did not exceed limit that would impair his efficiency, because his astute ma knew just how far to carry the instructions and what respc bility to attach to the situation in the presence of the work man. We may well imagine that if he had treated the matter lightly, the last diamond would have been broken too.

Many people fail because they overestimate the difficulty their undertaking, making it thereby more difficult because t have added from within a new and serious complication. Otl again fail because they underestimate the difficulty they hav grapple with, and thus they cannot meet all the conditions may arise unexpectedly in a given situation. Happy is the i who can so adjust his sliding-scale of energy as to avoid b extremes, for his shall be success.

4. The relativity of the degree of ease or difficulty as affe

by different *Aufgaben* is another corollary of our results that is likely to be of use in problems of practical education, if actually worked out systematically and amply tested. In the course of the preliminary experiments, when instead of presenting the stimuli at different intervals, different rates of speed were given for the same list, it became apparent that a 40 sec. list preceded by a 25 sec. list was easier to do than the same list either preceded by a 50 sec. list or given at the beginning without any preliminary list. The writer did not follow up this relativity phenomenon, seeing that it is only a side-issue with no direct bearing on the main problem, hence the suggestion comes only as a result of a few sporadic cases, but we cannot help noting it both because of its far-reaching application, if demonstrated, and, further, the comparative ease with which it may be tested on a large scale.

If verified, it would mean that if we are innervated for something that is altogether beyond us to accomplish, this effort, while useless so far as the infeasible task is concerned, will be of considerable service in tackling afterwards a less difficult piece of work, but yet, more difficult than usual. On this score, may it not pay then to set and sacrifice an *artificial* task that is too exacting in order to attempt another difficult task with greater alacrity than would otherwise have been the case? It seems that the prevalent habit of setting oneself a schedule that is consistently beyond the particular individual's capacity to keep up with is grounded in the principle that has just been suggested.

The question whether it is advisable to carry out such a plan in ordinary school teaching is a delicate one. It would certainly require a great deal of nicety and judiciousness on the part of the teacher to manipulate such a method; for though it might make things easier for the *average* pupil, it would tend, however, to lower the estimate of the teacher in the eyes of the pupils and to render them refractory, if he or she kept continually assigning burdensome work to them, only to make a considerable remission after a few minutes of consternation in the

class-room. Perhaps the best results are to be looked for in the case of self-imposed tasks.[127]

5. The retroactive inhibition of determining tendencies is another result the discussion of which will not be commensurate with its importance as applied to training. We have seen that according to this law, the determining tendencies weakened in a descending order of their specificity, i.e., the most specific determining tendency was the first to become suspended during inhibition, while the most general determining tendency was the last to be arrested during such a state, a phenomenon which puts us in mind of retrograde amnesia.

Now since the instructions or suggestions are the external conditions for these determining tendencies which are mediated through problem ideas, it goes without saying that it is the *most specific instruction that needs the closest attention and greatest stress.* Once the strength of the most specialized determining tendency is insured, the rest follows automatically, since every determining tendency, in coming into play, operates at the same time all those below it in specificity.

This does not mean merely that specialized work requires more practice than unspecialized, but that the amount of practice necessary is out of all proportion to their ratio of elaborateness. In other words, training should not end when it is carried to the *knowing* point, but must be continued till the *emergency* point. *It is the emergency that is the criterion of the strength of the determining tendency.* Only if it can stand the test there, has its hold been firmly fixed. This is a point the significance of which has not been universally grasped, though as a working principle it has been recognized from observation and experience in many quarters.

[127] If this fact should be confirmed after further experimentation, it would furnish us with the complementary phase of MEUMANN's Leistungsgesetz des Willens. The explanation of the process is partially contained in this statement: "Unter den Einfluss der Vorstellung einer grösseren Aufgabe spannen wir ohne bewusste Reflexion und ohne die bestimmte Absicht unsere Kräfte mehr an, als wenn uns eine geringere Aufgabe vorschwebt" (Vorlesungen, Vol. III p. 103, 2nd ed.) cf. also the important corollary as applied to education. loc. cit. p. 104.

Let us now turn to the individual differences and see what we can learn from the results appearing under that caption. We have seen that the subjects fall into distinct reactive types. There are the omitters and the confusers—subjects whose mistakes are those of omissions and subjects who, in addition to omitting, also make mistakes of commission, but then their output is much greater than that of the simple omitters. There are also what for want of a better name, we might call the *"undulators"* because they emphasize certain groups and leave gaps in others and the *"equalizers"* because they make an attempt at every group of stimuli, without completing it.

The question now arises whether we can put this result to the same use in educational psychology as the discovery of imaginal types. From an examination of the functional significance of the sensory process and the motor process, we must come to the conclusion that the two are incomparable. The aim of the sensory process is to gain *impressions;* that of the motor process is to give *expression.* Our individual imaginal tendencies can often be made serviceable in different ways according to the requirements of the thing we are trying to learn, but where such adjustment is not attainable, the individuals representing different types need not be exhorted to try to change their mode of learning by an effort to cultivate a different type of imagery.[128] We all recognize, of course, that there is a distinct advantage in being able to put all types of imagery into play, but ought the predominantly audile strive to become more of a visile? Ought the man with a great play of visual images seek to imbibe knowledge by making use of kinaesthesis? Considering that the end can be attained through any one of these media, it might be only a waste of energy to try to learn auditorily what might be done just as well visually, especially as there are so very few who can be said to employ one type of imagery to the exclusion of all other types.[129]

[128] In fact MEUMANN points out that there is more loss than gain in the attempt to cultivate another type of imagery than the one that a given individual is endowed with, and cites his own case as an illustration. cf. MEUMANN: Vorlesungen, Vol. II. p. 547, 2nd ed.

[129] J. R. ANGELL: Methods for the Determination of Mental Imagery,

It is different, however, with the motor process where in the first place, it is more *desirable* to be able to control one's mode of reaction, and secondly, it is more practicable in this sphere.

Since the two pairs of reaction types are mutually incompatible, it is clear that you cannot obtain the same result by following either mode of a pair. If speed is aimed at, the omitter will fail lamentably, while if accuracy is the goal, the confuser will find his work counting for very little. In our experiments either omitting or confusing largely had to take place, owing to the conditions imposed by the method. Mention may also be made of a more or less balanced type in which a compromise is shown between the two extremes. For the same reason, it was not possible both to attempt every group and to complete it, with the result that the subjects fell into these different classes revealing a tendency to emphasize this or that mode of reaction.

Now that we know that such different types exist with pronounced dispositions to react in one way or another, it behooves us to classify subjects (children) on such a basis and to promote greater adaptability in accordance with the purpose of the task. And for this reason it may be pointed out that in dealing with scholars we ought to take these differences into consideration, i.e., to make allowance for them. At the same time, we must try to remedy the lopsidedness by developing a sense of proportion that could be applied in every particular case. The boy who thinks only of filling so many examination books and does not pay so much attention to the details of his answers will find himself hampered in the end, but so will the boy who spends a great deal of time on details fall short of success. He who can carefully balance both factors will, other things equal, take the lead.

Psychological Review, Monograph Supplements, vol. XIII, p. 82; also M. R. FERNALD: The diagnosis of Mental Imagery, Psychological Review, Monograph Supplements, vol. XIV, p. 130. More emphatically THORNDIKE: Educational Psychology, vol. III, p. 381 ff.

COLVIN and MYERS (Development of Imagination, Psychological Review, Monograph Supplements, vol. XI) seem to have reached the conclusion that our imagery changes as our ideas become more abstract and that we grow out of our visual imagery to a large extent.

Can such a transition be effected? Is it possible to adapt one's mode of response? The following illustration from personal experience will probably serve to answer this question.

The writer had been in the habit all through his primary and secondary education invariably to emphasize the first few questions of an examination paper at the expense of the others, nearly always omitting the last with a remark to the effect that the time is up. It was assumed that the examiner would be able to judge in what sort of manner the last question would be answered, if the examination period were extended. Now some examiners do actually make allowance for such pressure, but this is by no means a universal attitude on the part of examiners; and those who are skeptically inclined or who overemphasize brevity will naturally discountenance such a view, because in their opinion, the student should accept the instructions implied in the make-up of an examination paper—literally.

It was not till the beginning of his graduate work that the soundness of such a view suggested itself to the writer and he began cultivating a tendency for distributing his work to meet the time requirement, until the two opposed tendencies nearly balanced each other.

Examples of this sort may be multiplied, and undoubtedly similar adaptations are taking place constantly, but matters would be much facilitated if we could pick out the different motor types out of a class of pupils early in life for special guidance along this line.

B. Application to Ethics

It may seem somewhat out of place to introduce in an experimental essay some observations bearing directly on ethics, but just because such is the case, does it seem desirable to deviate from the conventional method even at the risk of eliciting a shrug of the shoulders from both ethicists and experimental psychologists.

It is a fact but too patent that psychology is deliberately steering clear of all contact with ethics, with the result that ethical writers who start out with a normative scheme in their mind will be apt to apply this normative method to psychological

phenomena as well. We can scarcely pick up a text book of ethics without coming across a number of sections dealing with what is often called the psychological prolegomena of ethics. Such terms as desire, volition, motive, intention, wish, decision, deliberation and a score of others of that kind are discussed at great length, and with good reason, for as LADD remarks,

"Ethics, so far as it can be rendered scientific is one of the sciences. Hence its dependence upon psychology and anthropology is to a certain extent absolute."[130]

The treatment of these terms is, however, highly speculative; and, in the main, we are given access to an account of what *should* be the case rather than what actually takes place. Since, however, psychological states and physiological activities are not constrained to fit into any normative scheme we are confronted with the spectacle of formidable systems of ethics being built up on sand-hills. The consequences are that both ethics and psychology are at a disadvantage.

The fault lies with both sides. For the moralist claims that ethics, in dealing with its subject matter has a viewpoint and method of its own; while psychology, at the present day, totally ignores the exigencies of a sister discipline and prefers to remain on safe ground, thus giving an opportunity to ethical writers of soaring high up in the clouds where no resistance is offered to the *logically* (and sometimes illogically) conceived facts about certain mental states.

The chasm must be bridged, if ethics is to be regarded as a science, and if psychology is to guard its own interests. It is, therefore, incumbent upon every experimentalist in psychology, when opportunity arises, to do his share towards clearing up general misconceptions, just as it is the solemn duty of every ethical writer to take into consideration the results of experimental psychology and not to rely merely on an account that is both antiquated and highly speculative. For ethics to ignore the findings of psychology in regard to certain mental states the understanding of which is essential for any ethical theory would be comparable with the situation where psychology should take

130 LADD: The Philosophy of Conduct, p. 32.

no account of the work done in physiology or where physiologists should totally disregard the facts established in chemistry and physics.

It is not often that an experimental result lends itself to extension into another sphere such as ethics, but since we can perceive the possibility of such an application in the present study, we shall not lay ourselves open to the charge that we are not practising what we preach.

In one of the early chapters of this book, it has been intimated that while our investigation on the conflict of impulses does not include the subject of ethical choice, it does, however, suggest a certain connection between the two problems. STOUT as we have seen, concedes that disconnected impulses interfere with one another in a quasi-mechanical [!] way, but denies this as regards deliberative action. This sharp demarcation-line, drawn between two types of volitional activity opens up a fundamental issue; for it leads us to believe that an ethical situation is *toto coelo* different from an experimental situation, to the extent that a principle involved in the results of the one would work just the opposite way in bringing about a given result in the other.

We have, in the course of our experiments, obtained as our most general result the fact that, in the event of interference, we proceed along the lines of least resistance, that is to say, we choose more frequently the easier of two movements. Can we reasonably expect this to be the case when interfering impulses become conflicting motives towards action? Judging from STOUT's view of the matter, we cannot; and, so far as the writer is aware, that seems to be the prevailing sentiment in ethical circles at least. STOUT quotes with approval a passage from JAMES, which is here reproduced in part.

"We *feel,* in all hard cases of volition, as if the line taken, when the rarer and more ideal motives prevail, were the line of greater resistance, and as if the line of coarser motivation were the more pervious and easy one, even at the very moment when we refuse to follow it. He who under the surgeon's knife represses cries of pain, or he who exposes himself to social obloquy for duty's sake feels as if he were following the line of greater temporary resistance."[181]

[181] JAMES: Principles of Psychology, vol. II, p. 548, quoted in Stout's Manual of Psychology, p. 717 (3rd ed).

STOUT adds afterwards

"There can be no doubt that Professor JAMES here desc the facts accurately."

but he fails to take note of another passage by the same au in which much light is shed on that *feeling,* and which w in a large measure, prove that the contrary is true. Her find JAMES saying

"We are, I think, misled into supposing that effort is frequent than it is, by the fact that *during deliberation* w often have a feeling of how great an effort it would tak make a decision *now*. Later after the decision has made i with ease, we recollect this and erroneously suppose the e also to have been made then."[132]

Have we not the clue to the whole situation in these two tences, and may not this analysis be applied in every cas deliberative action? Does not the underscoring of the "feel" in the passage quoted by STOUT instantly put us in of the solution JAMES had offered previously of the probl To our mind, the emphasis on the word "feel" implies *phenomenologically* the view is true enough, just as *phenom ogically* we are *free to will,* but as a matter of fact, this phen nal situation explains nothing. As a basis for exhortatio matters of conduct it may be of some use with a certain of people, but if our aim is to connect our data in such a as to be able to explain them, we find ourselves at sea on adoption of such a doctrine as STOUT champions.

It is true that JAMES does not seem to be decided on whole matter, and he is not at great pains to conceal his et bias,[133] but since we are beginning to realize more and that the course of psychology cannot be steered by ethical tives, we must insist on psychological facts being settled wit the aid of ethical considerations.

To the popular mind, virtue is associated with the diff course of action. Of course the belief is universal that

[132] JAMES, loc. cit. pp. 534-535.

[133] Cf. what he says about freedom loc. cit. vol. II, p. 573. "The pr writer does this for the alternative of freedom, but since the grounds o opinion are ethical rather than psychological, etc. . . . ."

easier to yield to a temptation than to resist it, but this belief cannot be accepted as it stands. It requires further analysis. If it means that the average man or woman more frequently goes astray than not, then the view is certainly not a correct one. What is at the root of this belief is the fact that all people find it easier to yield to a temptation at times than *never* to give way to one. That is an entirely different story. The important point, however, is that whether we yield or not, we are *following the lines of least resistance.* Such a conclusion does not seem compatible with the conventional view of morality. One might object that it is putting the hero and the coward, the saint and the moral reprobate on a par. We ask: How else can it be psychologically? The hero is actuated by *his* idea just as the coward is determined by his. What distinguishes their mode of behavior is the judgment of value that attaches to each of their ideas respectively. That difference in significance, however, takes us into another sphere entirely. It is no longer a psychological fact but an axiological datum.

JAMES is inclined to judge the situation from the popular point of view which is only one-sided and anything but critical. Thus we are told

"He [the virtuous man] speaks of conquering and overcoming his impulses and temptations. But the sluggard, the drunkard, the coward, never talk of their conduct in that way, or say they resist their energy, overcome their sobriety, conquer their courage, and so forth."[184]

Now it doesn't take much acumen to perceive that behind this phrasing there lurks a *petitio principii.* Given the terms sluggard, energy, drunkard and sobriety, coward and courage, we cannot connect these pairs by the verbs *resist, overcome* and *conquer* simply because common usage precludes such a construction. But on the other hand, we can conceive of a case where the hustler overcomes his energy, the teetotaller, his sobriety, and the foolhardy man, his courage. Hence the argument from common parlance (a form of the *argumentum ad populum*) does not take us very far, and might as well be ruled

[184] Loc. cit. vol. II, p. 548.

out of court; for the psychological process corresponding to "resisting," "overcoming" or "conquering" does not favor the laudable impulse any more than the culpable one. It is we, *as judging conduct,* who have decided to couple the words bearing a connotation of effort with the acts expressive of virtuous conduct, because we have been accustomed to associate virtuous conduct with the feeling of effort, to begin with. And JAMES himself has already partially explained, as we have seen, the genesis of this association. Probably this association has been reinforced also by education and other sociological factors (religious lore, moralist propaganda).

Now let us take the very illustration adduced by JAMES and examine it in a psychological light. What happens when the hero or the martyr suffers for duty's sake? There are two courses open to him, and yet he chooses the most difficult one, we say. Yes, but his statement must be qualified by adding the further clause, "From our point of view as judging spectators and from the agent's point of view prior to his becoming an agent." At the moment he actually made his decision, he *acted in the line of least resistance.* Suppose the impulse to cry out under the surgeon's knife were one of the motive ideas and the idea of duty the other, then if the agent did not cry out, it is only because there was less resistance in that direction, that is to say, because the idea of duty was more insistent than the idea which would, under other circumstances, have given vent to a moan. It is similar with the moral reprobate who suddenly becomes converted. Much as the moralist may shy at such a view, it is nevertheless true that the actual conversion follows along the lines of least resistance. The idea that led to the conversion must naturally have been a very powerful agency in order to render the other course (which we naturally think as the easier of the two) more difficult. Why then leave it out of account?

The failure to place the two contesting motive ideas on an equal footing seems to be another case of not seeing the wood for the trees. Duty is such a comprehensive term that often it is lost sight of in the explanation of a moral act. One might

suppose that duty, reason, and the like, were not motive ideas or mainsprings of action from the way in which they are systematically eliminated from the determining process. In matters of conduct, this tetragrammaton is often a more potent conjuring-word than the Cabbalistic tetragrammaton. It is not necessary to engage here in a rhapsodical discourse on the potency of duty; but it does seem surprising that those who extol it most are the very ones to overlook its part as a determining idea.

This being the case, it is not to be wondered at when STOUT finds it difficult to square the "greatest resistance" doctrine of ethics with the demands of psychological determinism.

"If volition is merely the outcome of preceding psychological conditions," he says, *"it must follow the line of least resistance* [italics ours] but in the cases described it follows the line of the greatest resistance. This would seem to imply the intervention of a new factor."[185]

The difficulty is solved by showing that in the cases described, or any others that might be adduced, action is *not* in the line of the greatest resistance, and therefore no new factor is needed.

One might well understand how such a conclusion would militate against the supposed interests of morality. If both moral and immoral conduct follow the line of least resistance, it will be urged, then the ethical man is no more virtuous than the scoundrel. This objection which rests on a naive and conventional view of morality reminds us of SCHILLER's pointed gibe at the austere formalism of KANT's ethical doctrine, when he makes the scrupulous inquirer complain

> Gerne dien ich den Freunden, doch thu ich es leider mit
> Neigung
> Und so wurmt es mir oft, dass ich nicht tugendhaft bin,

only to find that the solution, presumably on KANTIAN principles, is

> Da ist kein anderer Rath, du musst suchen, sie zu
> verachten
> Und mit Abscheu thun, wie die Pflicht dir gebeut.[186]

[185] STOUT: loc. cit. p. 718.
[186] SCHILLER, in the series of epigrams known as "Die Philosophen."

The widely-accepted belief is that every act that is not immoral must be a burden; else it would not be regarded as a virtuous act. We should probably all be disposed to agree that "Ehrlich leben ist schwerlich leben," in the sense that it is very rare that one gets along without ever committing an act of dishonesty. But we must remember that the difficulty does not enter into the act proper. What we mean by saying that such a life is difficult is, psychologically speaking, that in cases of temptation, the idea of duty or dignity or that-honesty-is-the-best-policy is with most people, not likely to be always sufficiently vivid to overwhelm the opposing idea. Once, however, that idea does become focal, the act proper, which is interpreted as being honest, issues smoothly out of the idea. The difficulty then does not attach to the act, but to the idea becoming sufficiently vivid.

Hence it follows that all the emphasis must be shifted from the act proper to the antecedent idea or purpose.[137] The aim in ethical education, according to this presentation, is to see that the *desirable idea shall be the effective one*. Herein lies the value of the ARISTOTELIAN doctrine of ἕξις. Our problem then is to develop a single comprehensive idea, under which all or nearly all desirable acts can be subsumed, in such a way that it would be uppermost in critical cases, thus smothering the opposing idea.

After all, is this not precisely the course that moralists are taking when they tell us that we must make an effort and that we are then, in consequence of that effort, free to will the proper thing? Is not this exhortation calculated to enliven and reinforce that idea which is to realize the moral act and to smother the propensity idea? Is this not the secret of Kant's regulative Ideas?

The speculative moralist of the old school says

"You must throw off your evil propensities in order to be virtuous."

[137] *Cf.* in this connection, Holt's remarkable treatment of ethics along the lines of Freudianism and in physiological terms: The Freudian Wish. p. 59 ff.

The scientific moralist puts it this way:

"If you will regulate your life according to a comprehensive and thoroughly consistent maxim of conduct, no fighting will be necessary, because those evil propensities will be so vague and fleeting even if they do occasionally show themselves, that they will vanish before they can be attended to. The set purpose to resist evil tendencies only serves to throw them into greater relief and to emphasize them."

It goes without saying that, on such a plan, applied ethics is a part of education.

C. Psychological Interference and Efficiency.

In approaching the application of our experiment to industrial efficiency, we may recall the two classes into which the manager of a certain steamship company divided inefficient sea-captains. Some failed because they took so much time to decide on a certain course of action; others proved incompetent because they dashed headlong into the first plan that occurred to them, without gauging the situation as a whole. These two types are obviously not confined to the domain of navigation alone. Those who come in contact with a great many people every day, have the opportunity of observing these contrasts daily. In describing the two opposite types, we may not always employ the same terms on different occasions, yet the ideas meant to be conveyed by these terms are markedly similar. This man, we say, is impetuous or impulsive; another deliberate. Or we differentiate the resolute and the irresolute, or again, in more striking cases, we speak of the one as a DON QUIXOTE and of the other as a HAMLET.

All these pairs, we find, are colored by value references.[188] They are subjective expressions rather than objective descriptions. On the basis of our experiments we should be inclined to call the two types, the dynamogenic[189] and the inhibitive. Such

[188] JASTROW's transparent exposition of the two types is not free from this bias. cf. his Character and Temperament, Chap. VI.

[189] *Dynamogenic* is here used in its etymological sense, and as suggested by the division of PATRIZI who found in connection with (what he regards as) fatigue experiments, that with some subjects, mental activity, carried on during muscular contraction, will lessen the ergographic curve for that period

a classification, though primarily suitable to designate the extreme modes of behavior under conditions of interference or other sort of pressure, may, however, be applied generically equally well. Subject LV may be regarded as typical of the dynamogenic tendency, and subjects ZL and SR as characterizing the inhibitive tendency. Both are extremes, and like all extremes, they meet in a certain respect, viz., their unfitness for certain purposes, though they may be highly desirable for others. We may further assume that these two different tendencies, while coming into play in everything expressive of two given individuals, will reach their maximum expression just where the situation is gravest or most delicate. We may not be able to distinguish two individuals with respect to their impulsiveness or hesitancy in ordinary matters, but confronted with a difficulty to be solved in a short time, these people will fall into their proper spheres naturally.

The application to questions of efficiency resolves itself consequently into two subsidiary problems: (a) What method can we devise, in the interests not only of the employer, but of society at large, in order to detect the degree of a person's susceptibility to interference? (b) How can the individual involved improve his lot by avoiding excesses in either direction? The first problem, it will readily be seen, is of a *diagnostic* character; the second has a *curative* bearing, and involves more directly the persons in question.

As to the diagnostic phase of the problem, MÜNSTERBERG has held out high hope for his situation test[140] as a gauge of ac-

(type *interferent* ou inhibiteur), with others, it will heighten the curve (type dynamogène); and rarely there occurs an alternation of two modes, cf. M. L. PATRIZI: La simultanéité (Dynamogénie et Inhibition) entre le travail mental et le travail musculaire volontaire unilatéral ou symétrique. Arch. Italiennes de Biologie, LVII, 1912, pp. 176-177.

From an article by Ch. Féré (Mouvements Volontaires, Rev. Philos., XXVIII, 1889, p. 58) it appears that the term dynamogénie had already been in vogue since the appearance of BROWN-SÉQUARD's reserches on inhibition, but that usage corresponds to our "facilitation," i.e., it represents the *absence* of inhibition, but not an *exaggerated* tendency in the opposite direction, such as PATRIZI's usage implies.

[140] Cf. MÜNSTERBERG: Psychology and Industrial Efficiency, p. 86 ff. Also J. W. BRIDGES: An Experimental Study of Decision Types and their correlates. Psychological Review, Monograph Supplements, Vol. XVII, pp. 51-53.

curacy and rapidity in coping with a complex situation. It seems, however, to the writer that the situation test cannot answer the purpose for which it was originally devised, for it is too much of a *static* affair. The subject takes up the card and has to gain an impression of the preponderance of a certain vowel among a large number of miscellaneous letters. He does not, however, feel the *imminence of an impending danger.* The situation is the same at the first second as at the third or fourth second, his action or inaction during the first second being without influence on the situation as a whole. What we miss is the *dynamic* aspect. The switchman or the bridge-tender or the sea-captain has a constantly *changing* situation before him, when the critical moment arrives, which very fact makes a material difference as regards his state of mind and reactions.

In the material used in the present investigation, the dynamic character of the situation is very conspicuous; and it occurs to the writer that, the interference coefficient might be measured by administering tests based on the method employed in the present study. Instead of using two sets of stimuli, the number might be increased to three or four, while the rate might be reduced so as to be within the limit of one's purely motor ability. If the inhibition index alone should be desired, it could be obtained by comparing the motor coefficient (in tapping tests) and the coefficients revealed by the test proper, which would include several factors, the central one, however, being that of interference.

II.

When we come to examine the second question, i.e., how the individual can make the most of a situation by "keeping his head," we see at once that the matter presents many difficulties. In the first place it is necessary to get at the *modus operandi* of inhibition, and before going into the theoretical phase of the subject, let us at this stage only point out that *functionally inhibition means the loss of adjustment.* In a difficult situation we have to keep continually adjusting ourselves to the exigencies of the moment. It does not matter whether we are participating

in a smart social function or whether we are called upon to manage a refractory mob or whether we are placed in the position of a sea-captain on a perilous voyage. What is required in all these situations is a quick readjustment perhaps from moment to moment. In some people this readjustment takes place more easily than in others. Interference conditions will affect those persons least. They are the skillful, the dexterous, the adroit, the tactful, the well-poised and self-possessed, in short, the ones who are scarcely ever embarrassed and who seldom show signs of awkwardness. Others, again, who have adjusted themselves for a certain occasion, cannot make the transition to another adjustment so easily. The break is sudden and they stall, or else—and this is true of another class—they readjust themselves badly, that is to say, they fall into a maladjustment by recovering from their condition too soon. They have forced their motor set, and they behave as if they were under the influence of a drug. These are the people who commit a rash act, who in their contempt of embarrassment become forward or rude. They are affected by the complex situation as much as the others are, but in a different way.

What is it that hinders the easy transition from one adjustment to another? The fault, we should venture to say, lies in the excessive amount of imagination and play of ideas that the inhibitive type of individual is subject to. Every novel situation is greeted with a volley of associations which makes one lose his perspective. On account of this imaginative tendency, the motor attitude is not properly set to begin with, but is in a state of instability. Thus the slightest change in the situation would be apt to break the adjustment.

It is evident that the economy of imagination and association is most conducive to easy readjustment. Just enough is to be in consciousness as can be of aid in making the transition from one act to another. All the rest is irrelevant and defeats the end. Thus the speaker, who, when heckled by one of his listeners, attends only to the question and takes up a definite attitude, say of sarcasm, which brings on the desired associations, will probably manage to hold his own and carry his purpose.

On the other hand, however, if he should begin wondering what right his interrupter had to ask the question, or whether he ought to rebuke him or reply in an agreeable tone, the previous adjustment is broken, and the next one must be started all anew.

It is not from the lack of imagination that the inhibited person is at a disadvantage, but rather from the too great flight of imagination. This state is most typical of the adolescent period or the "Backfisch" age; and it seems from various accounts that the keener the intellect the more prone is the boy or girl to present an awkward appearance. In his delightful autobiography of youth,[141] ANATOLE FRANCE tells us of a trivial incident during his school-days that preyed on his mind for months. A woman he had adored, while visiting at his parents' home, had asked him, after playing a nocturne, whether he was fond of music; and all he could answer was "Oui, Monsieur," when what he wished to tell her was that she could draw sighs and sobs, that she could actually draw tears out of the piano. Here was an inhibition evoked by a single glance of the woman that so charmed him. The effect of this inhibition upon him can be seen from the following words

"Puisque la terre ne s'entr'ouvrit pas en ce moment pour m'engloutir, c'est que la nature est indifférente aux voeux les plus ardents des hommes."

Had the young PIERRE NOZIERE (as he calls himself) known at the time of the incident, as he came to realize later, that Madame Gance was only a coquette who could make a fine impression, he would not have made the slip in the first place, and, secondly, he would not have been affected by the slip in the way he was. Yet this same youth who could not get adjusted easily to a social situation felt perfectly at ease in dealing with an intellectual situation. At school, he was self-assertive and displayed a great deal of dexterity in handling delicate questions connected with his studies. His imaginative bent was serviceable there to the utmost, but in the social *milieu* it only distorted the elements of the situation.

The upshot of this is that the curative phase of our problem

[141] Anatole France: Le Livre de Mon Ami: XI

is fraught with much uncertainty. We have still to learn the psychological differentiae of the intellectual, the physical and the social situations. If, for instance, we find that the factor so helpful in the one situation proves to be a disadvantage in another, it will have to be decided first whether we want to curtail the influence of that factor. In a similar connection, JASTROW reminds us that

"It is in part because such persons are peculiarly sensitive that they *are* artists and creative individuals"[142]

the reference being to artists, poets, musicians, writers, inventors, students, the bearers of responsibility.

If there is some doubt as to the *desirability* of such a cure, there is even more misgiving as to its *feasibility*. No one will deny that as we grow in experience, we are apt to handle novel situations in a more matter-of-fact way. With every new experience of a similar sort, the situation becoms less suffused with imagination or non-practical ideas, thus enabling one to take up a practical attitude, that is to say, react upon it objectively; but in our "truth-is-stranger-than-fiction" world, something unusual may turn up at a later date, and he reverts to his old impressionableness. That is what, we might suppose, happens in the case of people intrusted with great responsibility. The captain of a vessel, the railway engineer may have thoroughly inured themselves to their responsible positions, but it is not often that they encounter a real danger. Some day, however, they are suddenly confronted with a grave peril, and then such an idea may occur to them as "To think I am in this position" or "That's frightful" or he may picture to himself the result of the calamity—and the moment he drifts into this affective attitude, he is stunned for action. When he recovers from this state, it is probably too late. That is, of course, on the supposition that the individual in question is of the inhibitive sort.

The transference of training undoubtedly helps us to overcome our susceptibility to inhibition on important occasions, but whether one can completely outgrow an inborn trait like

[142] JASTROW: Character and Temperament, p. 321.

that is a different matter. Mosso[143] may have learnt to address an audience without those symptoms of fear and embarrassment which were noticeable at his first public appearance, and ANATOLE FRANCE, we should suppose, managed to lose his ill-at-easeness when in female society,[144] but, if originally of the inhibitive type of mind, novel social situations would still present more difficulties to them than to the originally self-possessed and adroit individuals. What if Mosso should suddenly espy his king among the audience, or ANATOLE FRANCE should find himself unusually honored by his fellow academicians! We must also remember that age and fame tend to lessen the inhibitive tendency,[145] and, therefore, the average individual given to inhibition is not to be compared with the two men just mentioned in regard to overcoming that tendency.

Such popular injunctions as "Be calm," "Don't feel nervous," etc., etc., are not helpful at all, just because the inhibitive person who is struck with the delicacy of a situation is not master of himself, and cannot control his dulled condition. Again, if he makes up his mind beforehand to treat the situation, no matter how grave or delicate it might be, as a mere trifle, the chances are that he will lapse into the other extreme and do something rash.

Much is to be unravelled and analyzed before we can approach the therapeutic phase of our efficiency problem satisfactorily. We have only gathered up the loose ends of the tangled skein without attempting to take the threads apart, but obviously this must be done first before we can reach a conclusion applicable to the practical side of our investigation.

[143] Mosso: Fear, pp. 1-4.

[144] "Maintenant, je n'ai pas trop peur de mes contemporains," he says by way of contrast.

[145] ERNST RENAN seems never to have freed himself from this tendency. In his Souvenirs d'Enfance et de Jeunesse, which he wrote at the age of sixty, he says "Ma nullité avec les gens du monde dépasse toute imagination. Je m'embarque, je m'embrouille, je patauge, je m'égare en un tissu d'inepties" (chap. III).

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THE FREUDIAN DOCTRINE OF LAPSES AND ITS FAILINGS

By A. A. Roback, Harvard University.

Whether Freud's interpretation of lapses constitutes an integral part of his system or not, of one thing we are certain, and that is the growing importance of that phase of psychology as throwing light on every day occurrences, which, insignificant in themselves, are often the vehicles of portentous events.

We seldom notice a slip unless it assumes the proportions of a blunder. Sometimes we are prone to attach an ominous significance to such an occurrence, which fact might account for the halo of mystery surrounding the subject in semi-intellectual spheres. Aside from that, however, there is something fascinating about a situation where one acts, as it were, in spite of himself. It is our intention to say one thing, yet we say another. We purpose writing a certain word or phrase; our fingers, however, execute a set of movements other than those intended. What is at the root of this mischief.

The current phrase " forcing the hand " expresses the feeling rampant among the common people—as if the act were of extraneous origin. It is represented as the precursor of either a fortunate or an unfortunate event. Thus it takes the form of a symbol announcing the advent of the happening proper.

It is now evident that here we touch on Freudian territory. No one, of course, would think of imputing to the Freudian School the primitive view just referred to. Freud would be the last man to trace a slip to extraneous causes or to regard it as indicative of a future good or evil. But it is *symbolism* nevertheless which connects his doctrine with the crude belief of the man in the street. The lapse according to Freud and his followers, is symbolic of the ***hidden motives*** at work in the mind of the person guilty of the slip. The slip might then be said to manifest itself as an interference between the open intention and the underlying motive, the former being misplaced by the latter.

Curious as it may seem, Freud's doctrine of lapses has been rather immune from the darts of his many critics, though of all his doctrines it is the only one which is amenable to experi-

mental investigation. Aside from the posi
may be invoked in criticism of his conclus
in his account that is impugnable on the s
sense.

If asked to sum up in a word the failii
interpretation of lapses, one might say it i:
In most cases emphasis is laid on irrele
essential facts are ignored. Freud takes a
direction, to begin with, and leaves no
explanation.

The writer does not wish to convey the
is belittling Freud's valuable service in (
the contrary, he believes that Freud has
in psychology,[1] but it happens that in his ea
every human act under his all-embracing dc
to a number of simple facts. Some of th
the "Psychopathology of Everyday Lif(
gasp. Not only is there not the slightest
tentative explanation, but every bit of *vra*
sionally lacking, unless we assume that hi:
the class of people characterized by tl
toëvsky's and more particularly Artzibas
if so, the question arises as to what
worthiness can be attached to them. Can
think that a woman patient who was "
self for having made a slip of the tongu
her uncle was, by saying, "I don't know,
in flagranti," when what she meant to say
passant—can we imagine that she would t
self the next day of the possible imputatic
an educated person by reproducing "a re
which being caught *in flagranti* played tl
And the conclusion is that "the mistake (
had therefore anticipated the recollection,
had not yet become conscious."

That is not all. The critical reader wil
to suspect that certain characteristics and
ally read into the poor patients *ex post fact*
"who is the domineering spirit in her hor
because her speech lapse fits in with such

[1]A. A. Roback. *Menorah Journal*, vol. v., 191(
[2] Freud: Psychopathology of Everyday Life, 1
tion). The passage is poorly translated. It shou
which the fact of being caught *in flagranti* figure
[3] Freud (Brill) *loc. cit.* p. 91

Another woman "who was especially weighed down by the financial burden of her treatment" is probably credited with this circumstance by Brill,[4] just because she actually said, "Please do not give me *big bills*", when she meant to say " big pills." Brill attributes the lapse to the patient's circumstances. We prefer to take the simpler course and say that the mistake was due to a repetitionary assimilation in the direction of the easier sound, as it requires less effort to articulate a "b" than it does a "p."

The Freudians have not learned the truth of Occam's principle "*entia non sunt multiplicanda praeter necessitatem.*" They rather seem to put a premium on the introduction of as many factors as possible—and to no purpose; for, entertaining as the stories and anecdotes are, some of them self-incriminatory,[5] they *do not tell us anything about the mechanism of the lapse.* Has it ever occurred to Freud and his own, that in most instances cited where positive and negative concepts been unintentionally interchanged, the fact *is that in practically every case the lapse is brought about by the elimination of the prefix un or in?* The reason for this is that in the excitement of speaking or writing, we fasten our hold, often anticipatorily, on the stem of the word and not on the prefixes, which are separable and which in themselves are not significant. The important part is the "fortunate" and not the "un,"[6] the "selfish" and not the "un,"[7] the "responsible" and not the "ir."[8] Stekel and Freud may make much ado about a slip where "in eigennützigster weise" was written instead of "in *un*eigennützigster weise,"[9] but rather than say that the author of the slip was actually accusing himself of selfishness, why not look into the formation of the word misspelled? Might we not expect, in the case of a clumsy compound, drawn out in the superlative form, at least the lopping off of the negative particle, especially when there are already two n's in the stems of the word? It is really a matter of omission, and as abundantly proven, it is the particles and less important words that are omitted in haste, excitement, embarrassment and so on. The inexperienced public speaker will find that the substantives come to him readily but the prepo-

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[4] Freud *loc. cit.* p. 103.

[5] The self-incriminatory character of these allusions is so striking that they might be regarded as manifestations of a sublimated exhibitionism.

[6] For the references in this connection *cf. loc. cit.* p. 425.

[7] *loc. cit.* p. 126

[8] *loc. cit* p. 10[illegible].

[9] Freud, Psychopathologie des Alltagslebens, 2nd German ed. [illegible]

sitions are rather tardy. Stoll, too, has reported[10] that in the copying of the meaningful texts, the words most omitted were those that were of secondary importance. From our own results, we can extend this observation to cover letters, for as compared with the large consonants (letters like j. k, p. f, etc.) the vowels, semi-vowels, and small consonants were slurred.

The upshot of this criticism is that Freud and his associated are not warranted in attaching a hidden meaning to such lapses on the strength of uncritical and one-sided observations. If the writer intended to say that many of the Freudian arguments are inane, and his interpretations inept, and wrote instead of inane and inept, the stem-components "ane" and "ept," Freud might have had some slight ground, on his principle, for assuming that at bottom we are in sympathy with all his doctrines. Our protest would not be so loud in such a case against the assumption, *because the words "ane" and "ept" are not used without the negative particle.* Hence it would mean the breaking up of a graphic habit—a disruption which requires explanation. This concession, on our part, however, would not hold if the word intended were "inapt," for "apt" is a common word in every-day use; and so the lapse would originate in accordance with our previous explanations. The only case of a *lapsus calami* where the larger negative form "unhappy" was substituted for the intended word "happy" is given by Ernest Jones[11] who was told by a lady that an "old friend in writing to her closed the letter with the curious sentence, 'I hope you are well and unhappy.'" According to Jones, "the slip of the pen was evidently determined by his dislike at the thought of her being happy with some one else" as he had entertained hope of marrying her himself. Even this solitary case pointing apparently to the operation of a wish complex may easily be explained by supposing that the slip was occasioned by the conflict not of *two wishes*—the one censored and the other primitive—but rather by the interference of a *wish* and a *fact,* the mental attitude corresponding to the expression. "I hope you are well and happy, though I am unhappy." In other words, the antithesis is not between two states in the future, but between the future and the present. There is no need of positing even here a hidden wish mechanism. Most likely, the writer was actually thinking of his unhappy condition at the time he was concluding his letter,

[10] J. Stoll: Zur Psychologie der Schreibfehler, Fortschritte der psychologic, etc., vol. II, 1913-14, pp. 22-48.

[11] E. Jones: Papers on Psycho-Analysis: p. 63.

and the various imaginal elements that went to make up his mental attitude, were probably synthetized by the word "unhappy."

There are, nevertheless, two or three accounts of a *lapsus linguae* in Freud's (Brill) *Psychopathology of Everyday Life* that we can become reconciled to, but only after definite qualification.

Brill relates of an admirer of Roosevelt who remarked to his host on the occasion of an evening dance,

> "You may say what you please about Teddy, but there is one thing he can always be relied upon; he always gives you a square *m*eal." [12]

What he was about to say was a "square *d*eal," but the guests had reason to be disappointed in the hospitality of the host, and this embarrassing slip gave away the situation.

We have no difficulty here in accepting Brill's implication, provided it is admitted, on the other side, that the speaker had been *thinking* of the word "meal" that evening and probably the idea came up several times and, that too, suffused with emotion, as in the case of every hungry man. Hence there was with him what we should call a "predisposed attitude." Now the fact that square is used with both meal and deal, and that the two words are so similar in sound much helped the "predisposed attitude" to bring about the association and finally the unfortunate slip.

In the same way also is it understandable that a person who entertains high ideas of his importance would sometimes substitute an "I" when he is referring to other people, especially those he looks up to. Here we may say that a "prepotent attitude" (congenital) is at the root of such lapses.

Freudians will probably seize with great avidity on a passage in ex-ambassador Gerard's *My Four Years in Germany* as tending to corroborate their complex doctrine of lapses; and, for their benefit, the passage is reproduced here.

> "Monday afternoon there was a Bierabend in the large hall of the Yacht Club at Kiel. The Emperor was to have presided at this dinner, but his place was taken by his brother, Prince Henry. Sir Edward Goschen, the British Ambassador, who was living on one of the British battleships, sat on his right and I sat on his left. During the evening a curious incident happened. The Prince and I were talking of the dangers of after-dinner speaking and what a dangerous sport it was. In the midst of our conversation someone whispered to the Prince and he rose to his feet, proposed the health of the visiting British Admiral and fleet and made a little speech. As he concluded, he said, addressing the officers of the British fleet. 'We are sorry you are going and we are sorry you came.'" [13]

[12] *loc. cit.*—— p. 102

[13] James W. Gerard: [illegible]

What we should insist upon, however, in opposition to Freud and his school is that emphasis be laid on the *actual association* in the speaker's or writer's mind between the word intended and the mis-expression. The association most frequently is, but need not necessarily be, verbal. It may be kinaesthetic, organic, and what not, but pregnant with meaning just the same. Thus we are not obliged to resort to unknown and hidden unconscious forces that are constantly distilling sexual and other complexes.

On the subject of writing mistakes, Freud's material is rather tenuous, this time not only in quality, but also in quantity.[14] There are really very few illustrations here of genuine writing lapses, as Freud does not take the trouble to separate out the memory lapses, and deals with several kinds of mistakes indiscriminately. Yet when we do come across a *lapsus calami* proper we find much ado made about it. He takes two pages to account for a slip of his that could be done to much better advantage in a few lines. Here is the instance. Wishing to draw some money from the bank, he glanced at his account, and finding that it was 4380 crowns, he decided to bring it down to the round sum of 4000 crowns. Upon making out the cheque, however, he noticed that he had written 438 instead of 380 crowns.[15] Common sense and experimental evidence[16] would lead us to believe that while his attention was occupied with something else, the determining tendency to substract 380 was in abeyance. Hence his action incorporated the elements of perception of the figures and the marginal idea of subtraction. Some figure was to go. Now in a number like 4380, what impresses one is the 438, not the cipher. The 438 is more like a unit. It has more significance, besides the 0 comes at the end, and in many of our computations, for instance, in division, we have occasion to lop off the cipher at the end. Under the determining tendency to subtract mentally, this mathematical stereotyped act reinstated itself and the 0 was disregarded making the number 438, which was promptly copied on the cheque.

In the opinion of the writer, this explains amply what has taken place psychologically, but Freud naturally must introduce a whole series of numbers and weave into his material

[14]A rather significant fact when we consider that Freud recognizes together with Wundt, there is a greater tendency to make slips of the pen than slips of the tongue (cf. his *Psychopatholgie des Alltagsleben*, 2nd. edition, p. 56.)

[15] *Loc. cit.* pp. 122-123

[16] *cf.* A. A. Roback: *The Interference of Will-Impulse, Psych. Rev. Monogr.* Supplements vol. xxv, 1918, p. 91ff.

several interesting, though irrelevant, stories to account for such a comparatively simple operation.

Playing with numbers is a pastime much relished by the Freudians. In the Chapter on Determinism we are treated to a veritable number-salad which does honor to the old cabalists and mystic commentators of the Pentateuch, for in *their* exegesis, they were guided at least by a system of hard and fast rules. The commentary called Ba'al ha-Turim by *Jacob ben Asher* of the 14th century, has much in common with Freud's number-juggling, and it may be said that many of the *Notarikin* and *Gematrioth*[17] as they are known, contain sex allusions calculated to illuminate certain episodes in the Bible. Sometimes it would even seem as if Freud's race-fellow of the fourteenth century has proven himself the more ingenious of the two in that respect.[18]

From Freud let us pass to some of his devotees and see what they have to offer in the way of solving the problem of language interferences.

When a student writes April 11, 1911, instead of April 22, 1911, what simpler and more plausible explanation may we resort to than the fact of motor anticipation? Jones[19] rightly observes that "the date he actually wrote was the 11th, was no doubt influenced by the presence of these integers at the end of 1911." If, however, such a simple explanation were allowed to stand, then what would become of the Psychopathology of everyday life, which must be fed on illustrations of this sort? Hence the narrator interposes with a "but." The explanation is discounted by the psychoanalyst's saying: "But it is to be noted even in this connection that his mistake consisting [sic] in writing them earlier than he should, i. e. in putting the date earlier." Surely this offers no cause for mystification. Anticipation is just as legitimate a mode of making a slip as repetition. But furthermore, if Professor Jones had only taken the trouble to investigate the material, he would have noticed that in 1911 there occur four downstrokes, furthermore, that the student had been writing, in dates, the number 22 as compared with 1911 in the ratio of 12: 365. Why, then, on the basis of graphic habituation, is

[17] *cf.* Jewish Encyclopedia under Notarikon and Gematria respectively.

[18] A. A. Roback: *Freudian Psychology and Jewish Commentators on the Bible.* Jewish Forum, Oct 1918.

[19] E. Jones Psycho-Analysis, [illegible]

it at all strange that the student should
earlier?[20]

The hypothesis that Jones put forth is t
student was unprepared for a certain exa
first week of May and wished, therefore,
But if a wish were really the determining
the substitution should have occurred not
month but with the *name* of the month, as
that to "be through" with an examination
with a student than to have the ordeal me

Every one of Jones's illustrations migh
out reference to any wish complex. Thu
write "Dear Fred" he substituted Freud
planation is quite clear and does not call
that Jones had entertained a hidden wis
terms with Freud. Jones himself admi
stances of the slip of the pen were extra
to its occurrence—the similarity in the
identification of the men, the occasion of
so soon after the other one and so on. T
about the admission is that Jones has not
that while he had been writing the word
if not thousands of times, not to mention
sions on which he had seen the name in pr
articulated it, etc.,—while the name Freu
the part of the sun in a solar system, t
Fred" was being used for the first time as
an old friend "whom I had always called

Another instance of stretching interpret
the following account again taken from
young lady was secretly engaged to a me
will call Arthur X. She addressed a lette
Arthur X, but to Dear Arthur X, thus e
to let all the world know of their relations

It is strange that in the section on mispri
published originally in a journal, a slip
though corrected afterwards, was not com
Psycho-Analysis. On the part of a Freudia
opportunity to make a case for the fav

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[20] It is very amusing in this connection that,
puzzled about the student "writing them (the i
he should," what should he himself do (or perha
fault) but make the very same kind of a slip;
"consisted in writing" he has it "*consisting* in

[21] Jones: *Loc. cit.* p. 65.

[22] Jones: *Loc. cit.* p. 65.

symptomatic as a "Fehlhandlung" and would, in its turn, require an explanation. The slip referred to consists of the word "brochure" being mis-spelt as "broschure." The sentence in the original article reads: "In a broschure of mine that appeared as a German translation, a mistake was made of a less unfortunate kind."[23] Were we as uncharitable as most of the Freudians are in their evaluations of phenomena, we might be inclined to say that the intercalation of the s in "brochure" was due to the submerged feeling on the part of the author that the pamphlet or its substance was obscure, but in order to forestall any such ingenious interpretation, might we not suggest that the slip was made on account of the German setting produced by the circumstances described in the sentence quoted?

The following up of writing lapses by Freudians, who in their very comments make more slips thus affording the opportunity for subsequent writers of the same school to make additional observations incorporating ever new slips—this whole process is a veritable "comedy of errors." Let us give an illustration of this entertaining game.

In his papers on Psycho-Analysis, Jones had written that "distinctness of calligraphy is powerless to prevent such mistakes."[24] This statement occasions Holt to make the following observations: " 'Why *calligraphy*'? thought I, since of course calligraphy is necessarily distinct and Jones besides being a careful writer perfectly knows his classics. Of course he had unconsciously written 'calligraphy' instead of 'chirography,' because of the delicate boast which is thus conveyed, that his handwriting is always, even when indistinct 'beautiful.' To this extent his ego-complex had eluded his censor.. This was too good to lose, so at the bottom of the page I wrote in pencil, with reference to 'calligraphy' above, 'Should be 'chiro'—: Another case of *Verschreiben* [*lapsus calami*] *w.* odious cause.' And then the joke was on me. I had fully intended to write 'obvious' and was as astonished to see 'odious' as if another person had written it."[25]

Well, it was my good fortune to see Professor Holt's note in the original, and I may say that in his pencil note the word 'chiro' appears as 'cheiro,' so that in the very copying of the note, Professor Holt, who is punctiliously careful in such matters, had made a slip which was not corrected. But further-

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[23] Jones: The Psychopathology of everyday Life: "*American Journal of Psychology*, vol. XXII, 1911, p. 503.

[24] E. Jones: Papers on Psycho-Analysis [illegible]

[25] E. B. Holt: *Th*[illegible]*an Wish* [illegible]

more, the slip "odious" for "obvious" may easily be explained on another and simpler ground than the one furnished by Holt. Those who have access to that particular book, which is in the philosophical library of Harvard University, will find that the word 'should' in the note was written directly above the word 'obvious.' There are certain letters in words that stand out and catch the attention more strikingly than others, and the 'd' of 'should,' written as it was on that part of the page, is one of these letters. Professor Holt must have seen the d of 'should' just as he was beginning the o of obvious and copied it unintentionally; and being, as a rule, more critical than positive in his attitude, the word 'odious' has probably just about as many chances of occurring to his mind as the word obvious.

But the climax is yet to come. In spite of all the explanations and observations and inferences, Professor Jones has made no slip in the first place; for though he may "perfectly know his classics," he seems thoroughly grounded in his English, too, at least enough to be aware of the fact that there are good precedents for using "calligraphy" in the sense of mere handwriting, regardless of its aesthetic value. Had he written 'chirography' instead, he might have been accused of delicately boasting his perfect knowledge of the classics, even if he should not have laid himself open to the charge of pedantry.

Thus we see (1) that the supposed slip was a perfectly correct usage and should not have called forth the "odious" complex; (2) the substitution of the word 'odious' for 'obvious' is happily explained, and (3) the real lapse—that of writing 'chiro' for 'cheiro' as it appears in the original note—was not recorded.

A misprint in which the Austrian prince was referred to as a "*g*egenstiftender Mittler" instead of "*S*egenstiftender Mittler" occasions A. J. Storfer[26] to go into certain details bearing on the political relations between Austria and Hungary. If Storfer had only laid his history aside and examined the sentence more closely he would have seen that in the words "beruhigt den gegenstiftenden, etc.," the substituted "g" is an assimilation caused either by the perseveration of the "g" in *beruhigt* or by the anticipation of the "g" in the word *Segenstiftenden,* or both, the perseveration determining the anticipation.

[26]A. J. Storfer: *Zur Psychopathologie des Alltags Intern. Zeitschrift für ärztliche Psychoanalyse,* vol. II, 1914, p. 172.

In expressing his condolence to a young woman who had recently lost her husband, someone had employed the words:

> " Sie werden Trost finden, indem Sie
> sich völlig Ihren Kindern widwen."

This apparently is a case of repetitionary assimilation, partially determined by the consciousness, on the part of the speaker (or writer), that he was addressing a widow. Th. Reik's[27] interpretation of this perfectly harmless slip is that the suppressed idea alluded to consolation of a different sort, viz:

> " eine junge schöne WITWE wird
> bald neue Sexualfreuden geniessen."

Comment on this is unnecessary.

At times it looks as if the "complex" mechanism were to operate in a prophetic manner. The minor Freudians never allow themselves to stop before any extravagance in drawing their inferences. Thus the mere substitution of a D for a P in the word "Epithel," by a person whose acquaintance with an Edith was only superficial at the time of the slip but had afterwards become intimate, elicits the following explanation from the author: "Das Verschreiben ist also ein hübscher Beweis für den Durchbruch der unterbewussten Neigung zu einer zeit wo ich selbst eigentlich davon noch keine Ahnung hatte, und die gewählte Form des Diminutivums Charactersiert gleichzeitig die begleitenden gefühle."[28]

As usual, we are told everything that may have a remote connection with the origin of the lapse, but the sentence in which the word "Epithel" was to have occurred is not given. As to the dimunitive form(?) revealing the embryonic affections of the writer who was apparently not aware of them at the time they "accompanied" the slip, it is enough to say that the explanation is highly ingenuous and amusing. In all probability, the mistake was caused by either an anticipatory or perseverative assimilation, and *Edithel* was no more of a diminutive form than Epithel is. The assimilatory process may of course have been touched off because of a previous association which had gained a certain amount of vividness and, therefore, impressive force, but even so, we are not warranted in ascribing an oracular function to the subconscious mechanism presiding over slips of the pen.

An ante-bellum allusion to the great European conflict is

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[27] Th. Reik: *Fehlleistungen im Alltagsleben intern. Zeitschrift für ärtzliche Psychoanalyse,* vol. III, 1915, p. 44.

[28] Richard Wagner: Ein kleiner Beitrag zur Psychopathologie des Alltagsleben: *Zentralblatt für Psychoanalyse,* vol. I, 1911, p. 594

mentioned by Emil Simonson[29] who cites the report in a German newspaper of an interview given out by the then Russian minister of foreign affairs, Sazanov, to the *Novoye Veremya*. In this interview, Sazanov was quoted as discussing the friendly relations between the Triple Entente and the Dual Alliance (Zweibund), but as Italy, at the time, had been one of the members of the Alliance, to which the minister was referring, the quotation raised some comment in German political circles.

A diplomatic malapropism of this sort leads Simonson to remark that there was uncertainty somewhere as regards Italy's place in the Triple Alliance; and if the slip was made either actually by Sazanov or someone else on the Russian side, the likelihood is that it was caused by a feeling that Italy might be induced to part company with Austria and Germany, while if either the German correspondent in Russia or the newspaper in Germany was responsible for the lapse, the incident is to be explained by the fact that its author was distrustful of Italy's attitude and secretly harbored the belief that it would be better for Germany and Austria to remain a dual alliance.

Simonson's expectations, or rather knowledge of the situation turned out to be correct, but, interesting as this bit of information may be, it is irrelevant to the matter in hand unless we know definitely whether Sazanov has made a slip in the first place or whether it was his intention merely to stress the original core of the Alliance. Besides, in the report of an interview, so many different agencies are involved that we should only be groping in the dark for a foothold until we can trace at least the source of the lapse.

The Freudian complex distillery does not appear to be so productive in French as it is in German, yet we must not suppose that the French language is immune to misinterpretation, and, for the sake of variety, we might look into a couple of French versions of the Freudian story with a bit of Swiss local color.

R. Weber,[30] to begin with, gives illustrations of lapses occurring with the inmates of the institution with which he is connected. The *lapsus calami* which has been singled out as especially characteristic is one made by Weber himself when

[29] Emil Simonson: Ein interessanter Fall von "Versprechen" oder "Verschreiben." *Zentralblatt für Psychoanalyse*. Vol. II, 1912, p. 363.

[30] R. Weber: Petite Psychologie. *Archives Internationales de Neurologie*, 1912, reviewed in the *Zentralblatt für Psychoanalyse*, Vol. II, p. 536.

he wrote instead of *Peillard*—the name of a patient who was known for his wantonness—the word *Paillard* which is the vulgar expression in French for "rake." A pun of this sort is nothing but natural, but if Weber was not really aware of punning at the time he made the slip, we need only recall the fact that the word *paille* and its derivatives are so commonly used in French, while the word *peille* is not to be found in the dictionary—to understand the cause of the slip. The mistake, then, is only the expression of a motor habit.

Maeder[31] reports an elaborate bi-lingual lapse. In the first place, he could not, according to a dream of his, find the French word for "gauze," and so he asked for it in German, using the word *Mull,* but when he read over the dream he recorded, he found that instead of *Mull* he had written down *moule*—a word a friend of his family was in the habit of using as an equivalent for "blockhead."

The explanation unfolds a long series of associations which give us a roundabout clue of the origin of the particular lapse.

Much less satisfactory is an account of a lapse offered by H. Hellmuth.[32] *Levitico* (Wasser) had been substituted in a prescription for *Levico* (Wasser); and we are supposed to believe that at the root of this slip was the desire on the part of the physician to have his patient dress *quickly* and make room for another patient. Hence he had incorporated the French word "vite" (quick) into the mis-spelled German word (!). When it comes to fanciful explanations, the female Freudians are evidently not to be outdone by their male colleagues.

That it is not difficult to hit upon a Freudian interpretation can be seen from the following facts. Claparède[33] relates how one day he wanted to administer some laudanum to a patient, but asked his assistant to hand him the bottle of tincture of iodine. A careful introspective account follows as to how the mistake originated. He traces this lapse back to an associative dynamic which he would call mechanical, passive, or superficial in contradistinction to Freud's intentional, active or deep-seated dynamic. In another case, analogous to this he had said "bismuth" when it was his intention to say "magnesia." His explanation of this is similar. The visual

[31] A. Maeder: Psychologie de la Vie Quotidienne. *Archives de Psychologie,* vol. 7, 1908, p. 288.

[32] H. Hellmuth: Beiträge zum Kapitel "Verschreiben" und "Verlesen." *Zentrabllatt für Psychoanalyse,* Vol. II, 1912, p. 277.

[33] Ed. Claparède: De la représentation des personnes inconnues et des *lapsus linguae. Arch. de Psychologie,* vol. XIV, 1914, p. [illegible]

image of the two powders is the same (white) and governs two words with the predominance of the one of these visuo-verbal associations over the other. Are we to suppose, Claparède asks, alluding evidently to Freud, that I had a secret desire to constipate the patient who needed a purgative? And picking up a psycho-analytic journal, what do we find but that Ferenczi,[34] Freud's adjutant is inclined to accept Claparède's *reductio ad absurdum* question as the proper solution of the lapse.

Finally, we have still to analyze the case referred to by Menzenrath who repudiates the doctrine of Freud, and yet comes very close to adopting Freud's method and certainly the instance we are about to cite is a typical Freudian interpretation, much as Menzenrath may deny it.

Menzenrath[35] had begun writing to a friend,

> "Dass Sie bald nach . . . zu kommen
> gedenken, macht mir besonders Vergnügen,

but instead of besonders he wrote beden—i. e., he had started to say "Bedenken" which he explains as a complex due to the fact that the pleasure was not "particularly" great, and the whole note was an idle conventional form. That may well be, but Menzenrath had not noticed that the word "gedenken" preceded the unfinished "bedenken." Here we have an assimilation pure and simple. Furthermore it is unlikely that the word begun was *Bedenken,* for then it would have been written with a capital letter. It is apparent that the word was begun as "besonders," but as soon as the stroke of the "e" in "be," was finished, there was a shift of attention, and as "be" is similar to "ge" in sound, and both are prefixes of *"denken,"* it need not at all seem odd that the *denken* of *gedenken* should reverberate while the mind was wandering.

In a later discussion,[36] Menzenrath shows still less sympathy with Freud's "universal hypothesis," though he thinks it is impossible on the part of adversaries to disprove Freud's contentions on introspective grounds. That Menzenrath has exaggerated Freud's invulnerability on the matter of speech and writing lapses, will, it is hoped, become evident from our present study.

The trouble with the Freudians is that instead of seeking

[34] S. Ferenczi: *Internat. Zeitschrift für ärztliche Psych.* Vol. III, 1915, p. 123.

[35] P. Menzenrath: Psychopathologie de la vie journalière, *Archiv für die gesamte Psychologie,* vol. XXIII, 1912, p. 510. Report of the VIe Congrès belge de Psychologie et de Psychiatrie.

[36] P. Menzenrath: Fehler des Alltagsleben: *Kongress für Experimentelle Psychologie,* VI, Göttingen, p. 57

an explanation in the context, in the immediate mental content, they soar off into the realm of the fantastic for their search, and return with a far-fetched interpretation. *As a methodological principle in the study of lapses, the writer would lay down the rule that first the word, sentence, or sentences preceding or following are to be examined,* then we must look into possible associations that may have determined the mistake; only in default of such clues, would it be legitimate to hunt for a new principle of explanation.

The Freudians scarcely ever give us the whole context of a lapse, but prefer to concoct a solution of the home-made brand and, it may be added, for home consumption. In the *Psychopathology of Everyday Life,* there are a number of acute observations, but in a great many statements there is neither rhyme nor reason, and in general Freud's doctrine must be taken *cum grano salis.* To be consistent, he should hold that *a workman who misses his step on a high scaffold and falls into space has a secret desire of ending his life; for in what way is the psychological mechanism of such a slip any different from others that bring no grave consequences with them?*

Similarly, the person who has taken poison by mistake—let us say the toper who reached out for the bottle of wood alcohol instead of the rum—is afflicted with a *taedium vitae.* In fact, many serious accidents, countless misfortunes are caused by slips. Are we to conclude, then, whenever some evil befalls us that somewhere in our nervous system there is a hankering after the mishap? Much of our illness is due to what is ordinarily spoken of as thoughtlessness. We escort our friend to the door on a cold night and unmindful of the fact that we have just left an overheated room, we stand in the draft conversing until we catch cold. Where disease is contracted through infection or contagion, lapses are nearly always at the root of such contractions. Yet we shall have a difficult task finding the man who may be said to invite illness, however unconsciously, unless as very rarely happens, when illness is a boon alternative as compared with some greater evil that may be in store for him.[37] As a rule, however, there does not seem

[37] *Cf.* A German Deserter's War Experience, p. 163. . . "We were especially forbidden to make use of woolen blankets, because the French were infected with scabies. 'Scabies' is an itching skin which it takes at least a week to cure. But the order had a contrary effect. If one was the owner of such an 'itch blanket' one had a chance of getting into the hospital for some days." . . . "In the evening we took some of the forbidden blankets, hundreds of which we had captured that day . . . everybody wanted to get the 'itch,' however strange that may sound."

to be the slightest motive for wishing to fall ill or for courting death Even the motives that are thought to produce the questionable Oedipus and Electra complexes cannot be exploited in this connection.

Freud has been so fortunate in the authority he wields among his devotees that his utterances are accepted uncritically. On the other hand, most of the criticisms directed against Freud have come from men who dislike his doctrine on general principles, but who have not taken the trouble to subject his illustrations to a rigorous examination. It is on this very account that we have spent so much time on the discussion of Freud's analysis of lapses. His doctrine should further receive its *coup de grâce* by the trend of our experimental results[38] which show that lapses have been made in the hundreds; lapses in numbers, nonsense syllables, characters of the alphabet and other symbols. Shall we posit hidden complexes for each of them? But a given word or syllable may have been misspelled in many different ways by the same subject. The whole idea of looking for unconscious motives in experimental work of this sort seems too absurd to barely mention. In what way, then, are the lapses occurring in everyday life to be regarded as of a different material?

The principle of *determinism* which forms the background of the Freudian theory of lapses is sound, of course, *per se*. No scientist would deny that the writing lapse is conditioned by certain physiological or psychological antecedents. But what right has one to *create* a cause when the direct antecedent is in most cases apparent. Occasionally, as Wundt has recognized, there may be a complication of causes which it would be almost impossible to disentangle, and psychical influences may be at work,[39] but Wundt is by no means a Freudian even in the matter of speech lapses, though Freud eagerly seizes on a quotation from the *Völkerpsychologie* as a pretext for dragging in "Saul among the prophets," and by italicizing the phrase *"following the principle of the complication of causes"* in the passage quoted from Wundt the Viennese psychopathologist has evidently attached a special significance to Wundt's words, thus leaving the reader mystified.[40] In this state, the latter is apt to believe that Wundt has made an important concession to Freud.

[38] This study, which will shortly be published, constitutes the second part of the *Interference of Will -Impulses* previously referred to.

[39] Wundt, Völkerpsychologie, die Sprache, Vol. I, part I, pp. 361-382. 1900.

[40] Freud: Psychopathology of Everyday Life (English translation). pp. 79-80.

Well, then, let it be pointed out that the quotation mentioned has been broken off abruptly just where Wundt proceeds to illustrate precisely what he means by "a complication of causes." It certainly has no reference to any suppressed wishes or complexes, but to the *ordinary processes of association,* stimulated perhaps by the initial syllable of another word, or to several of the processes, already explained, taken collectively.

Not the slightest trace of adherence to, or rather anticipation of, Freud's principles can be detected in Wundt's discussion of lapses; and it is idle to read into his views such an interpretation. Moreover, it occurs to us that if Freud misinterprets a simple passage written in his native tongue, then how much more prone is he to put a wrong interpretation on phenomena as variegated as they are elusive? And if the master is apt to err so profoundly in an ordinary matter, then what shall we say of the epigones who lack his penetrating insight and generalizing ability as well as his systematic grasp of the phenomena generally classed under the subconscious?

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THE "ALL OR NONE" LAW IN VISUAL RESPONSE.

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I. INTRODUCTION.

Modern investigation of the response of neuro-muscular tissue under stimulation indicates that this response follows a so-called principle of "all or none." If a normal nerve or muscle fibre is set into action at all, it is necessarily excited to the maximal degree which is possible for it. The minimal and maximal responses are identical. This principle was demonstrated for muscular tissue before it was shown to be applicable to that of the nervous system, but the researches of Adrian and Lucas indicate quite clearly that it holds for individual nerve fibres as well as for the individual contractile elements of a muscle.[1]

At first thought this principle seems to be incompatible with the existence of delicate gradations of neuro-muscular action. The explanation of the actual occurrence of such gradations appears to lie in the fact that any muscle or "nerve" consists of a large number of individual cells which all coöperate in a single function subserved by the whole. Gradations in the intensity or power of the response can thus be attributed to variations in the *number* of individual cells which take part in the process at any time. Small power in a muscular contraction is attributed to the fact that only a few of the muscle cells are active, while the remainder are wholly inactive. A more powerful contraction would involve a larger number of active cells. Similarly in a nerve trunk which supplies such a muscle an innervation of low intensity would consist in the maximal activity of a few constituent fibres combined with the total inactivity of the remainder. A more powerful innervation would entail the action of a larger proportion of the total number of fibres. Obviously, according to this view, there must be a limiting value to the intensity of any neuro-muscular process, which value is determined by the simultaneous activity of *all* of the cells of the system.

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[1] See Lucas, K. "The Conduction of the Nervous Impulse." London 1917.

Forbes and Gregg[2] have found that this principle actually applies to the "action current" of a nerve, but that gradations in the degree of the motor response which is set off by this same nerve current are still obtainable long after the limiting value of the "action current" has been reached. They explain this fact by the assumption that although the "all or none" principle holds for each fibre, the number of *impulses* reaching the muscle along a single fibre per second can vary. The response of nerve tissue is of such a nature that the nerve current must be *pulsatory* in character, since the excitation of the nerve cell is accompanied by a condition of reduced irritability, in which it is impossible to restimulate the nerve. This condition disappears *pari passu* with the state of excitation, so that a second "all or none" response cannot be evoked until the first one has died away.

The most convincing demonstrations of the "all or none" principle have been carried out on motor or efferent nerves. It is well known that sensory nerves differ in their properties from motor nerves, but it is very doubtful whether this difference can extend to such a fundamental property of nerve activity as that which is expressed in the "all or none" principle. The results of Forbes and Gregg substantiate this doubt. However, if we assume this principle to apply to the sensory nerve we encounter grave difficulties in the explanation of certain phenomena of sensation. The facts of psycho-physiology indicate very clearly that the intensity of sensation depends directly upon some aspect of the nerve processes in the cerebral cortex. This "aspect" in turn must depend quantitatively upon the intensity of the response of the receptors or sense organs which are involved. Consequently, the intensity in question must have some quantitative representation in the sensory nerve current. In the case of sensation we have to face the added difficulty that gradations of *quality* as well as quantity or intensity, are apparently transmitted from the receptor to the central nervous system.[3]

In certain departments of sensation, at least, it is possible

[2] Forbes, A., and Gregg, A., "Electrical Studies in Mammalian Reflexes. II. The Correlation Between Strength of Stimuli and Direct and Reflex Nerve Response." *Amer. Jour. of Physiol.*, 1915, Vol. 39, pp. 172–235.

[3] Similar demands would arise from a purely physiological study of the motor responses of the organism to stimuli of various intensities applied to sense organs.

to account for the afferent transmission of intensity by supposing that the number of individually excited fibres in the sensory nerve varies with the intensity of the receptor activity, so that the intensity of the sensation is determined at the nerve centres by the total number of fibres which are pouring impulses into that region. This would be in harmony, for example, with many of the facts of auditory response as interpreted by the theory of Max Meyer.[4] According to Meyer's view the loudness of a tone depends upon the number of fibres of the basilary membrane which are set into action, each such fibre being responsible roughly for the excitation of a single nerve fibre of the eighth nerve. The pitch or quality of the tone would be accounted for in accordance with Rutherford's theory [5] by the frequency of the nerve impulses sent along each or all of the fibres, this frequency being equal to that of the sound vibrations. According to this interpretation, frequency of impulses would be responsible for transmission of quality to the nerve centres, while the number of individual sensory fibres which were excited would be responsible for the transmission of intensity. In the case of the cutaneous and kinesthetic sensations, it would also seem possible to account for gradations of intensity in terms of the number of individual fibres stimulated, while variations of quality seem to depend entirely upon the identity of the nerve path which is stimulated. Phenomena of gustatory—and possibly olfactory—sensation are apparently consistent with a similar interpretation if we suppose that the separate receptor cells have varying thresholds, so that more of them are excited by a strong stimulus than by a weak one.

When we come to consider the case of visual sensation, however, as I have pointed out in a previous paper,[6] the phenomena which we encounter are much less easily explicable in terms of the " all or none " law. In this department of sensation the number of retinal elements which are stimulated appears to determine our consciousness of the *area* of the object, and all intensities of sensation seem to be elicitable from a single rod or cone. It is true

[4] Meyer, Max, " Zur Theorie der Differenztöne und der Gehörsempfindungen überhaupt." *Zeitschr. f. Psychol. u. Physiol. d. Sinnesorg.*, Bd. 16, S. 1–48, esp. S. 22 ff.

[5] See Schäfer, E. A., " Text-Book of Physiology," 1900, Vol. 2, pp. 1190–1192.

[6] Troland, L. T., " The Nature of the Visual Receptor Process." This JOURNAL, 1917, Vol. 1, esp. pp. 11–12.

various intensities of excitation of the former could be translated into corresponding nerve impulse frequencies passing along the individual optic fibres. This theory, I believe, satisfactorily explains the facts of pure luminosity vision in harmony with the "all or none" principle, and also DuBois-Reymond's law. It appears, however, to leave no plausible way of accounting for the transmission of the qualitative or chromatic aspect of the visual excitation. If we correlate intensity with the frequency of the nervous impulse, color must depend upon something different. It is generally believed that all possible qualitative variations in visual sensation are elicitable from a single retinal cone, provided the latter is located in the zone of trichromatic vision. The original "*Dreifasertheorie*" of Young and Hemholtz has been universally discarded, although it seems to the writer (*vide infra*) on somewhat inadequate grounds.

The difficulties which we encounter in attempting to reconcile the facts of both achromatic and chromatic vision with the "all or none" principle are so great that we might conclude that, after all, this law is incompatible with the facts of visual response and cannot be accepted as applying to the action of the optic nerve fibres. It is well known that the optic nerve is, strictly speaking, a brain tract and not a sensory nerve in the strict sense of the term. Consequently we might feel warranted in attributing to it, properties different from those possessed by other afferent conductors. The observations and measurements to be described below, however, present new empirical data which it seems to me can only be interpreted as demonstrating the actual operation of the "all or none" law in visual response.

II. DESCRIPTION AND THEORY OF THE BLUE ARC EFFECT.

If a spot of red light surrounded by a dark field be viewed with the right eye under favorable conditions, one will see streaming off to the right of the spot two arc-shaped bands of slightly violet luminosity. These bands converge upon a portion of the visual field which measurement shows is identical in location with the blind spot. The exact form and separation of the two bands varies with the position of the stimulus spot with respect to the centre of vision; when the spot is directly fixated the bands tend to fuse together, but when the fixation point is moved toward the right they separate constantly until they finally disappear.

If the left eye is employed an exactly symmetrical appearance is observed centring at the blind spot of that eye. In binocular vision the two sets of arcs can often be seen together.

This phenomenon has been called by Mrs. Ladd-Franklin the "blue-arc effect."[9] It was apparently first described by Purkinje,[10] who seems to have observed every possible entopic phenomenon. It has been rediscovered by a number of later observers, including Zeeman,[11] Gertz,[12] Hubbard,[13] and others. It appears, however, to be still practically unknown to the majority of psychologists and physiologists in spite of the remarkable significance which it acquires when its very obvious explanation—recognized in its essentials by previous investigators—is once appreciated. It also is unfortunately true that very few quantitative studies have been made of the effect. I have, therefore, thought it very well worth while to undertake such studies of various aspects of the phenomena, one of which I am reporting in the present paper.

In another forthcoming article,[14] I shall present measurements of the topography of the blue arcs for various sizes and positions of the stimulus spot. A comparison of the map, in terms of visual angles thus determined, with the distribution of the nerve fibres in the nerve-fibre layer of the retina—in the region surrounding the fovea and between it and the optic disk—shows conclusively that the effect is attributable to the action of these nerve fibres upon neighboring portions of the retina. The individual nerve fibres of the retina are unique in consisting of bare axis cylinders without either myelin sheaths or neurilemmas.

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[9] In a forthcoming paper.

[10] Purkinje, J. E., "Beobachtungen und Versuche zur Physiologie der Sinne," 1825. Bd. 2, S. 74–78. The entire passage is quoted by Gertz (*vide infra*).

[11] Zeeman, P., "Ueber eine subjektive Erscheinung im Auge." *Zeitschr. f. Psychol. u. Physiol. d. Sinnesorg.*, Bd. 6, 233–234.

[12] Gertz, H., "Über autoptische Wahrnehmung der Sehtätigkeit der Netzhaut," Erste Abhandl. *Skand. Arch. f. Physiol.*, 1907, Bd. 19, S. 381–408. Zweite Abhandl., *ibid.*, 1909, Bd. 21, S. 315–350. Also: "Ueber entoptische Wahrnehmung des Actionsstroms der Netzhautfasern." *Zentralbl. f. Physiol.*, 1905, Bd. 19, S. 229–233.

[13] Hubbard, J. C., "A Curious Secondary Visual Phenomenon Resulting from a Stimulation of the Macular Region." *Psychol. Bull.*, 1910, Vol. 7, pp. 196–199.

[14] To be published in *Psychobiology*.

Since they are also crowded closely together, especially in the region about the fovea, they are in circumstances particularly favorable to the transfer of excitation from one to another.

The mechanism of such transfer of excitation can readily be surmised. When the fibres connected with the retinal rods or cones of a small stimulated region of the retina are excited they suffer a so-called negative variation in their electrical potential. This variation would tend to cause a flow of electric current towards the nerve fibres in question, or at any rate a change—in the sense of an increment of the current flow in this direction—in any current which might happen already to be flowing. This change in current flow would constitute, in accordance with the DuBois-Reymond law of nerve stimulation, a natural stimulus to any nervous tissue through which the lines of current flow might happen to pass. If the excitation of the nerve fibres which were primarily involved is continued without change no further secondary stimulation of other nerve fibres should occur, since the DuBois-Reymond law demands stimulation only when there is a change in the magnitude of the electric current which is acting upon the nervous tissue. The demands of this argument appear to be met by the facts, since the blue arc effect is seen only at the moment of application of the primary stimulus and lasts approximately a second. Of course, in order to insure steady stimulation of the retina it is not only necessary that the external stimulus be maintained constant, but the eye itself must not be permitted to move. In another article I shall discuss the relation of the effect to the DuBois-Reymond principle in detail.

The form of the blue arcs for various regions of primary stimulation of the retina shows clearly that the primary nervous elements involved in the transfer of excitation are the nerve fibres which connect the region in question with the entrance of the optic nerve. More doubt exists, however, with regard to the exact identity of the secondary nervous elements to which the excitation is transferred. It is unlikely that these latter elements are other nerve fibres which run along parallel with the primarily excited ones in the nerve-fibre layer of the retina, since among such fibres there are probably included some which connect with regions of the retina not lying along a continuous path between the stimulated area and the optic disk. The sensations derived from the secondary excitation will naturally be "projected" on

that portion of the visual field which corresponds with the positions of the retinal receptors supplied by the nerve fibres in question, and the blue arc effect is always confined to the region of the visual field between the primary sensation and the blind spot.

There are two other possibilities regarding the identity of the elements which are secondarily excited. One is that these elements consist in the neurones of retinal layers external to the nerve-fibre layer and the axes of which are perpendicular to those of the fibres in the nerve layer. In general, a nerve fibre is much more readily stimulated by the interruption of an electrical current which travels along its axis than by that of one which is moving across its axis. This principle, therefore, would favor secondary excitation of the neurones in the external strata of the retina as compared with that of nerve fibres running parallel with those primarily excited. The final possibility is that the elements involved in the secondary excitation are actually the receptor cells of the retina themselves, although we are not certain that these cells are subject to stimulation according to the DuBois-Reymond principle. It is known, however, that they are in general much more sensitive than the simple nerve cells, and it is quite likely that such additional sensitivity would be necessary to permit one nervous element to pick up a residual effect of the activity of another such element. It should be easy to devise experiments capable of distinguishing between these three possibilities.

Since we attribute the blue arc effect to the stimulation of the retinal apparatus by an electric current, it is of interest to compare the color of these arcs with the colors obtained by the application of externally produced electrical voltages to the eye. The investigations of Helmholtz,[15] Müller,[16] and others show that if the electric current passes outwards along the line of sight the color seen is ordinarily a dark reddish-yellow, but that if it passes inward the color is a " whitish-violet." [17] This latter color is the same as that of the blue arcs, and a moment's consideration shows that the direction of the internally produced electrical current which we have assumed to be responsible for the generation of

[15] Helmholtz, H. von., "Handbuch der Physiologischen Optik." *3te Aufl.*, 1911, Bd. 2, S. 13–17.

[16] Müller, G. E., " Ueber die galvanischen Gesichtsempfindungen." *Zeitschr. f. Psychol. u. Physiol. d. Sinnesorg.*, 1897, Bd. 14, S. 329–374.

[17] See Helmholtz, *loc. cit.*, Bd. 2, S. 15.

this phenomenon is also the same as in this case of the external application of electrical forces. In the latter case the visual effect continues with continued application of the voltage, but is much stronger at the "make" than thereafter. This coincidence of relations appears to offer a striking corroboration of the explanation which we have given of the effect.[18]

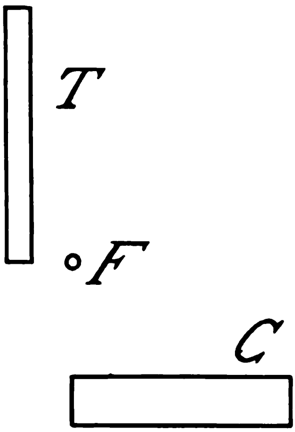
If the action of the nervous elements of the retina follows the "all or none" principle, as well as DuBois-Reymond's law, we should expect to find evidence of this fact in the blue arc phenomenon. The "all or none" relationship might be inserted at one or more points of transfer, in the total propagation process which is involved. In the first place, we should expect the action currents of the primarily excited fibres to be independent in magnitude of the intensity of the radiation acting upon the retinal receptors to which they were attached. Consequently, the potential difference generated by these action currents should be independent of the receptoral stimulus in question. In the second place, we might expect the secondary excitation of other elements to be of a magnitude independent of that of the current which is set up by the potential difference just mentioned. In case the secondarily excited elements are the receptor cells themselves this expectation might not be fulfilled, since it is certain that these cells do not have an "all or none" response to radiation or to many so-called inadequate stimuli. Even in this event, however, we should still expect to find evidence of the "all or none" character of the action current in the primarily excited elements.

It is, of course, certain that a large number of individual nerve fibres are involved in the primary excitation, but it is equally certain that the individual action currents of these fibres cannot summate to any appreciable extent electrically, at least as regards their voltage characteristics. In other words, the voltages which the individual fibres severally generate cannot be conceived to be arranged in "series," but must rather be conceived as arranged in "multiple." When a very small number of fibres are involved, a reduction in the secondary excitation effect might be expected, due to "leakage," but if the number of fibres is reasonably great, the density of the current effect will not be increased by a further

[18] I find, after writing the above, that similar arguments have been advanced by Gertz, in his very thorough and penetrating theoretical analysis of the basis of the blue arc phenomenon. *Loc. cit.* 1909 Bd. 21, pp. 327–350

to render rays from this plane substantially parallel for the observer's eye, which is placed immediately behind the artificial pupil.[19] This makes it easy for the observer to obtain a sharp image upon his retina of the stimulus field pattern, which is determined by the apertures in an opaque card placed in front of and in contact with the lens, *L*. The pattern of these apertures is shown, drawn to scale, in Fig. 2. Immediately behind the lens, *L*, is a shutter, *H*, consisting of two blades meeting at the vertical diameter of the lens but having a small opening coinciding in position with the central spot, *F*, of the stimulus field pattern. Between the lens, *L*, and the spectrometer slit, *S*, there is placed a screen, *N*, making an angle of 45° with the optical axis of the system and being of such a height as to intercept the light from the spectrometer only in the lower part of the stimulus field. This screen is illuminated by a small electric flash-lamp, *F′*, which is mounted upon a carriage running on a photometer bench at right ang'es to the optical axis in question. The important dimensions of the system are given in centimetres in the figure.

FIG. 2.



Pattern of stimulus field.

The functions performed by the various portions of the stimulus field pattern shown in Fig. 2 may be indicated as follows. The central spot, *F*, serves as a fixation point and is constantly

[19] The distance from the artificial pupil to the cornea of the observer's eye was 8 mm.

filled with light from the spectrometer even when the shutter is closed. The vertical slot, T, is also filled with light from the spectrometer when the shutter is open, but it is dark when the latter is closed. The lower end of this slot is located on the horizontal diameter of the field which passes through the centre of the spot, F. The horizontal slot, C, is filled with light from the screen, N, when the shutter is open, but is dark when the shutter is closed. The left-hand end of the last-mentioned slot is located on the vertical diameter of the field, which also passes through the centre of the spot, F. Both halves of the shutter, H, open simultaneously with extremely high speed when an electric contact is made.

TABLE I.

Dimensions of Stimulus Field.

Dimension (See Fig. I.)	Millimetres	Visual Angular Degrees
T, vertical	16.0	6.39
horizontal	1.5	0.60
C, vertical	2.8	1.12
horizontal	13.5	5.40
F, diameter	0.7	0.28
T to F, horizontal (neighboring edges)	2.2	0.88
C to F, vertical (neighboring edges)	7.0	2.80

The dimensions of the stimulus field pattern in millimetres and also reduced to visual angular degrees for the observer's eye are given in Table I. The angular dimensions were calculated by means of the formula,

$$\Phi = 2 \tan^{-1} \frac{mo}{2a}$$

where o is the linear size of the given field dimension, a the actual distance of the field from the eye, and m the magnification introduced by the lens, L'. This latter factor, in turn, was obtained by use of the formula,

$$m = \frac{fd}{fd - x(d - x)}$$

f being the focal length of the lens, d the distance of the field from the anterior focus of the eye, and x the distance of the lens from the same point. This magnification amounted with the given lens powers and other dimensions of the system to 1.315.

The slot, T, which is filled with spectral radiation of known wave-length constitution, furnishes the primary stimulation of the retina required to produce the blue arc effect. Fixation is maintained upon the spot, F, and for this reason only one, *viz.*, the upper, of the two arcs ordinarily seen is visible. Special tests with slot, C, dark, demonstrated the complete absence of the lower arc. With the given distance between T and F and for the writer's right eye, the upper arc shoots out from T to the right at a distance above the horizontal diameter of the field approximately equal to that of the slot, C, below this diameter. The slot, C, is seen filled with " white " (or "tungsten") light of low but controllable intensity, and the problem set for the observer is to vary the intensity of this light until it appears to be equal to that of the blue arc. In other words, the slot, C, furnishes a comparison field which permits the establishment of a rough photometric equation between the single blue arc and an objective standard.

The objective intensity required for this equation was found to be very low. To obtain the required low intensity it was found necessary to employ a small electric flash-lamp of the lowest obtainable candle-power on a long photometer bench and in addition to this to coat the screen, N, with lamp black having a reflection coefficient at the angle of reflection which was actually utilized of 0.0216. The small flash-lamp had to be operated at the low " efficiency " of 5.58 watts per candle, corresponding with a candle-power of 0.069. The lampblack surface was produced by smoking the screen, a brass plate, with an illuminating gas flame enriched by passage of the gas through benzol. This screen may be regarded, for purposes of the present investigation at least, as being non-selective in its reflecting power.

It is obvious that one of the most important conditions underlying the photometric equation which the procedure of the experiment establishes between the subjective blue arc and the white light of the slot, C, is the given state of adaptation of the visual apparatus. In order to have any hope of demonstrating the luminosity law of the effect we must be sure that the level of adaptation is constant. The most convenient method of maintaining such constancy is to work with complete dark adaptation, which may be assumed to represent the asymptote of the adaptation curve. For this reason the measurements to be tabulated below were made fairly late in the evening after the observer's eyes had

become adapted to vision at low intensities of illumination. The work was done under dark-room conditions and all extraneous light was screened from the observer's eyes during the tests. All of the observations were made by the writer and with the right eye. The writer's vision is, so far as he is aware, normal in all respects except for a certain amount of astigmatism, which, however, is very low for his right eye and was not especially corrected in the present experiments.

The experimental procedure was as follows: Eight ranges of the spectrum were selected which roughly divide the spectrum into equal intervals from a red ending at 680$\mu\mu$ and a violet ending at 420$\mu\mu$. The exact ranges of wave-lengths are shown in Table II and are accurate to about 1.0$\mu\mu$. Filters were employed in the orange-red and blue-violet ends of the spectrum to reduce the amount of scattered light present. It was desired to employ each of these eight spectral stimuli in the slot, *T*, of the stimulus field each at a number of different intensities. Previous experiments had shown that the threshold of the blue arc effect varies noticeably with wave-length and that its maximum value is about 5 photons.[20] Accordingly, multiples of this value increasing by powers of two, from 5 to 640 photons, were employed. It was not possible to obtain the highest of these intensities in all cases on account of limitations in the capacity of the light sources which were employed.

The intensities were established by means of flicker-photometric equations with the brightnesses of a magnesium oxide surface illuminated by a standard lamp at distances calculated to yield the required values. The area of the artificial pupil which was employed was, of course, taken into consideration in calculating the distances in question. This pupil was a square, 2.51 millimetres on the side. The reflection losses due to the four lens faces were also allowed for. It was not possible to obtain all of the desired illuminations of the magnesium oxide surface simply by varying the distance of the standard lamp from the surface, and it was therefore necessary to operate the lamp at various voltages which had previously been calibrated with respect to candle power. The flicker equations were made with a

[20] A photon represents the intensity of stimulation of the retina with a stimulus field brightness of one candle per square metre and a pupillary area of one square millimetre.

circular field 7.31 degrees in angular diameter, corresponding with a diameter of a diaphragm in the field of the lens, *L*, of 1.83 centimetres. They were made by the writer, using the same eye which was employed in the actual tests on the blue arc. The results of the flicker equations, between the various standard brightnesses thus established, were recorded in terms of the voltages required across the spectrometer lamp in order to secure the flicker equations. Owing to the general law of the phenomenon to be studied—its slow variation with respect to intensity change—it was deemed sufficient to make only two flicker equations for each stimulus of given wave-length constitution and intensity, provided the voltage readings checked within approximately 0.05 volts on a total range of 6 volts. The calibration of the spectrometer system in this way for the various levels in intensity was carried out independently of the actual observations on the blue arc effect so that fatigue of the eye due to the photometric determinations could not have any effect upon the main observations which were to be made.

On account of the large number of variations involved in these measurements it was wholly impossible to make observations representing all of the experimental settings in a single evening. The most convenient method was to go through the entire range of intensities for a single spectral region in one evening. This, of course, introduces some uncertainty regarding constancy of the various levels of adaptation which existed on successive evenings and hence for the different spectral stimuli. The influence of variations of this sort is probably responsible for some of the irregularities which appear in the results. However, our main interest in the present measurements lies in the law of the phenomenon with respect to intensity rather than to wave-length.

In general, five successive determinations of the apparent brightness of the blue arc were made for each intensity. The procedure in making these determinations was to fixate the spot, *F*, of the stimulus field and then to operate the shutter, *H*, which exposed the slots, *T* and *C*, simultaneously. A blue arc would be observed to shoot out from the side of *T* as the slot, *C*, came into sight, these two streaks of luminosity occupying symmetrical positions in the field of view. If the horizontal slot appeared brighter than the arc the small lamp illuminating the screen, *N*, was moved farther away from the screen to reduce the brightness of the slot.

or in the opposite direction if the latter appeared darker than the arc. The shutter was again closed, the point, *F*, refixated, and the experiment tried again until a satisfactory balance of luminosities was obtained. The eye was not exposed to the stimulus field any longer than necessary to make a photometric judgment, this time being approximately one second. In general, about five trials had to be made for each equation. The results of the equations were recorded in terms of the distances of the lamp from the screen, a knowledge of which would permit computation of the brightness or photon value of the slot, *C*, for the luminosity equality. The distances were in the neighborhood of one metre, and the candle power of the lamp was maintained very accurately constant by keeping the voltage across its terminals always the same.

TABLE II.

Apparent Brightness, P, of Blue Arc in Ten-thousandth Photons.

Wave-Length $\mu\mu$		Stimulus Intensity in Photons							
		5	10	20	40	80	160	320	640
683.3–700.0	P	16.79	18.0	21.0	20.10				
	A. D.	.74	1.1	1.7	.54				
646.3–660.0	P	31.2	30.22	28.8	21.5	22.76	30.28	29.90	
	A. D.	1.6	.92	2.2	1.7	.50	.93	.72	
608.8–620.0	P	16.86	16.55	18.37	19.80	20.40	18.9	22.02	31.4
	A. D.	.89	.81	.69	.28	.64	1.4	.47	1.5
571.0–580.0	P	11.68	12.22	11.70	13.20	14.94			
	A. D.	.17	.22	.20	.56	.94			
532.8–540.0	P	19.10	20.22	20.50					
	A. D.	.62	.97	.38					
494.4–500.0	P	14.92	16.7						
	A. D.	.16	1.2						
455.9–460.0	P	27.16							
	A. D.	.99							
417.4–420.0	P	25.36							
	A. D.	.95							

The first series of observations was made in five evenings ranging over a period of three weeks. The results of these observations are presented in Table II. This table shows for each stimulus condition the average (and precision measures) of the various photon values which were required in the horizontal slot to match the blue arc in luminosity for any given experimental setting. The units employed in the table are ten-thousandths (10^{-4}) of a photon. It will be observed that the intensity in question is extremely low, being only slightly above the absolute

threshold for the writer, under conditions of complete darkness adaptation. The incompleteness of Table II is due to several causes. For the wave-lengths 683.3–700.0 $\mu\mu$ it was not possible to obtain an intensity higher than 40 photons, and for the wave-lengths 646.3–660.0 $\mu\mu$ the maximum intensity obtainable was 320 photons. The incompleteness of the results for wave-lengths less than 620 $\mu\mu$ is due to an entirely different cause, *viz.*, the difficulty of observing the effect for these wave-lengths with high intensities of stimulation. This difficulty is not due to absence or weakness of the blue arcs themselves, but rather to the fact that with these wave-lengths the entire field of view is flooded with luminosity due to light scattered by the ocular media. The scattering is probably very little greater physically for these wave-lengths than for the longer ones, but the dark-adapted peripheral retina is highly sensitive to wave-lengths less than approximately 600 $\mu\mu$ and relatively insensitive to wave-lengths greater than this value. This disturbing effect of general peripheral stimulation, due to scattered light acting upon the rods, becomes worse with increasing intensity and also with decreasing wave-length, so that while at 580 $\mu\mu$ observations can be made up to 80 photons, at 460 $\mu\mu$ they can scarcely be extended beyond the threshold of the effect. It is often possible at higher intensities to see the blue arcs through the general haze, but it is practically impossible to make any sort of judgment of their apparent brightness.

A study of Table II shows very clearly that the blue arc phenomenon tends to follow the "all or none" principle. Consider, for example, the results obtained for the spectral range 608.8–620.0 $\mu\mu$. If we neglect the value of 31.4 obtained at an intensity of 640 photons, the increase in the apparent brightness of the arc in changing from a stimulus intensity of 5 to one of 320, involving a factor of 64, is from 16.86 to 22.02, involving a factor of only 1.31. Even this increase may be due largely to accidental variations, since the apparent brightness of the effect for 160 photons is only 18.9. The noticeable increase in the value of the effect for 640 photons is attributable to the appearance of the peripheral glare at this intensity, this glare apparently reducing the sensitiveness of the retina to the objective light which is used as a comparison. For the spectral range 646.3–660.0 $\mu\mu$ there is actually a general decrease in the apparent brightness of the effect with an increase in the intensity of the stimulus. This decrease

is probably to be attributed to a slight increase in the sensitiveness of the retina to the comparison stimulus, *C*, due to progressive dark adaptation occurring during the course of the observations. The change, however, is very small. Similar comments will be found to apply to the luminosity values of the phenomenon obtained for other wave-length ranges.

It will be seen that the general order of magnitude of the luminosity of the blue arc remains constant not only for different intensities but also for different wave-lengths. The variations which occur between wave-lengths are probably explicable as due to two causes: (1) existence of different levels of adaptation on the several evenings during which the observations were made, and (2) differences in the disturbing influence of scattered light in the eye for the various wave-lengths.

Having determined by the above described observations that the only stimuli which it is practicable to employ over a large range of intensities are of wave-lengths greater than about 600 $\mu\mu$, a second series of observations was carried out in which conditions were somewhat more carefully controlled. For these observations a spectral stimulus constituted by wave-lengths between 642.0 and 680.0 $\mu\mu$ was selected, since experience showed that this region of the spectrum does not readily arouse the peripheral glare. The spectrometer lamp was calibrated for intensities from 5 to 640 photons as in the first series of observations. Besides being made in the evening after four or five hours of twilight vision, complete darkness adaptation was further guaranteed for this series by a period of fifteen minutes' rest of the eye in total darkness before work was commenced. A minute's rest of the eye was also permitted between individual observations. A further possible source of variations in adaptation involved in the method of the experiment, consisted in the necessity of using the eyes between experiments to read the scales of instruments and to write down the values found. The brightnesses for this work were made as low as possible, approximately 0.16 candles per square metre. In the first series of observations some slight decreases in the sensitiveness of the eye to the comparison field were noted as a result of this exposure to light. In the second series after every appreciable exposure of this nature two or three minutes' time was allowed to elapse before a new observation was

made, and the regularity of the results which were obtained indicates that this factor had a negligible influence.

In order to guard against a possible effect of progressive changes in adaptation during the course of the work, two sets of observations were made. The first began with the lowest intensity, followed by progressively increasing intensities up to the highest. The second set was started with the highest intensity, followed by progressively decreasing intensities down to the lowest one. A progressive increase in adaptation during the series of increasing intensities might accidentally produce an "all or none" law for the phenomenon if the sensitivity of the retina to the objective light should happen to increase *pari passu* with an increase in the intensity of the arc. If the reverse order of intensities were employed, however, the adaptation change would tend to increase rather than to decrease the variation due to the change in the stimulus intensity.

TABLE III.

Apparent Brightness, P, of Blue Arc in Ten-thousandth Photons.

Order	Stimulus Intensity in Photons.								
		5	10	20	40	80	160	320	640
Increase.........	P	9.81	9.65	9.65	10.28	9.64	9.32	9.72	11.25
	A. D.	.16	.21	.21	.31	.14	.15	.18	.33
Decrease........	P	9.32	9.19	9.26	9.21	9.32	9.95	10.38	12.61
	A. D.	.15	.23	.11	.20	.18	.28	.45	.44

Wave-length Range of Stimulus, 642.0–680.0 $\mu\mu$

The results of the second series of observations are shown in Table III. These results leave no possible doubt that the blue arc phenomenon actually obeys the "all or none" principle. The constancy of the apparent brightness of the arc for intensities between 5 to 640 photons is remarkable, and any variations which occur can certainly be accounted for as a result of slight variation in the level of adaptation or simply the uncertainties of the general method of measurement. Indeed, considering the difficulty of making photometric matches under the conditions of the experiment the constancy of the values is surprising. It is

only for the intensity of 640 photons that variations outside of the limits 9.19 to 10.38 are found, and at the intensity in question a considerable amount of peripheral glare was present.

In connection with this second series of observations the absolute threshold was determined for the light in the slot, *C*, and found to be 8.1×10^{-4} photons. This value is 0.82 times the average value (9.910×10^{-4}) of the photon intensity which matched the blue arc in this series. In another series, involving less perfect dark-adaptation, a threshold 0.68 times the apparent brightness of the blue arc was found. These determinations of the limen were made with the same fixation as that of the blue arc measurement and in the presence of the spectral stimulus. It is evident that at least under the conditions of the observations here reported, the brightness of the blue arcs is only very slightly above the threshold. These results agree in general with those of Gertz.[21]

IV. METHODS OF COMPUTATION.

The method of computing the results given in Tables II and III was as follows: Each of the individual items of these tables represents five separate determinations of the objective brightness required to match the blue arc. The original records of the conditions for these matches were in the form of distances in centimetres between the screen, *N*, and the small flash-lamp, *F'*, which was always operated at a constant voltage. The five distances thus established for any given experimental setting were first averaged by finding their arithmetic mean. The illumination of the screen for this average distance of the lamp was then calculated by means of the inverse square law from the known candle-power of the lamp. This illumination value was then converted into units of brightness through multiplication by a constant depending upon the reflection coefficient of the screen, the angle of reflection, and the general optics of the arrangement.

This constant was derived from a similar constant which had been computed for the magnesium oxide disk employed in making the flicker photometric equations. The relation between the two was established experimentally by illuminating the smoked screen, at the position in which it was used during the tests, by a lamp of constant candle-power placed fairly close to it at a known distance and then equating to it a second luminous field obtained

[21] *Loc. cit.*, 1909, Bd. 21, S. 326.

from another light source so arranged as to permit of an equality-of-brightness comparison between the two. This secondary field was then maintained at the determined brightness and the magnesium oxide disk substituted for the smoked screen in the same plane. The source of illumination of this plane was then moved away until a photometric equation was established, this time between the magnesium oxide surface and the secondary comparison field. From the distances of the lamp from the screen and disk, respectively, the ratio of the brightnesses of these two surfaces for a constant illumination could be calculated.

The original brightness value coefficient for the magnesium oxide surface was calculated on the assumption of a coefficient of reflection of the surface in question of .863, supposing also that the surface obeyed Lambert's Law. On these assumptions the brightness of the surface would be expressed by the relation

$$b = \frac{ir}{\pi}$$

where i is the illumination of the surface and r is the reflection coefficient. The photon values given in the Table could be computed from the brightness values of the disk or the screen by multiplying the latter by the area of the artificial pupil which was employed in all of the measurements, and by the transmission factor of the system. For this purpose the area of the pupil would be expressed in square millimetres and the brightness in candles per square metre. For simplicity of computation, the pupil and transmission factors were naturally combined with the constant required to reduce the illuminations to brightness values, so that the photon values were obtained by a single multiplication.

It would, of course, have been desirable theoretically to have computed the brightness or photon values for the individual distances of the lamp from the screen and then to have averaged all of these separately computed values. The results obtained by this procedure, however, would not have differed sufficiently from those actually found to have warranted the large increase in labor which would have been involved by the theoretically more exact method.

Tables II and III show under the columns marked A.D. the precision measures of each of the main values of the table. These precision measures represent the "average deviations of the

averages" of the individually determined photon values. were computed in the following customary manner. The av deviation of the individual distance readings from their mean value was first determined. This deviation was then di by the $\sqrt{5}$ in order to obtain the average deviation of the distance reading considered by itself. This latter precision ure, Δm, was then substituted in the equation,

$$\Delta p = \left(\frac{2\,\Delta m}{m}\right) p$$

where Δp is the average deviation of the mean photon valu given in the tables, p the mean photon value, and m the mea tance reading for the given case. This formula was deve from the relation between the photon and distance values b usual method employed in calculating propagated errors. average precision of the main values in Table III, thus comp is 2.3 per cent., which is sufficiently low to account for the d tions from constancy of the values in question, at least of values which were obtained under conditions of substan constant adaptation and freedom from peripheral glare.

V. THEORETICAL CONSIDERATIONS.

The work reported in the present paper naturally req to be supplemented by further measurements of the blu effect upon other subjects. In the small amount of time wh available to me for research of this character I find it more i esting to endeavor to enter new lines of investigation th follow them up in great detail. However, I hope that o who are interested in vision and have more time at their dis will undertake to test the results reported in this paper u large number of subjects.

If further analysis of the cause of the blue arc phenom should justify the interpretation which has been placed upo the present article (and by Gertz) the "all or none" char istic of the phenomenon will have to be regarded as of f mental importance for the theory of visual response. We be obliged to face courageously the problem of determining it is possible for gradations of intensity to be transmitted t brain centres from individual rods or cones by an activit secondary influences of which exhibit no gradations whats

It seems almost certain that intensity must be represented in the nerve currents moving along individual fibres of the optic nerve by the total number of quantal ("all or none") pulses passing through a fixed cross-section of any one of these fibres per unit time.

No great difficulty would probably be found in reconciling the momentary character of the blue arc effect with the assumption that a constant stream of impulses is generated by the stimulus which sets off the effect in question. This effect must be produced by the action of a very large number of individual nerve fibres, and it is improbable that the pulses in all of these fibres would be exactly synchronized, so that after the first general rise in excitation of the mass of conductors no variations in their combined activities would be detectable to an outside sensitive structure. Another principle which might be involved is a possible difference in the natural periodicities of the primarily and secondarily excited tissues. The facts indicate that the retinal receptors have a much greater inertia for any change of state than have the nerve conductors. It should also be noted in this connection that the blue arc effect has a very distinct negative after-image which by itself might account on the basis of simple fatigue or adaptation for the momentary character of the effect, although I do not at present favor this as a complete explanation.

If we attribute intensity transmission to the frequency of the optic nerve impulses, however—as previously noted—there seems to be nothing left to explain the transmission of the two color characteristics, hue and saturation. Conceivably, these may be represented by some sort of "group frequency," although this seems a rather far-fetched explanation. It is difficult to imagine a mechanism in the cones or their junctions with the optic neurones which could translate the extremely high frequencies of the radiant stimulus into any kind of group frequency capable of being transmitted along a nerve fibre.

Of course, we have still much to learn with regard to the exact nature of the nerve impulse and it is possible that variations in the *form* of the pulse may occur without involving any change in its total magnitude. In this case we might endeavor to correlate transmission of the chromatic characteristics along the nerve with certain aspects of the form of the individual pulses. However, the improbability of this hypothesis leads one

to wonder whether there is not some chance of being able to return to Helmholtz's original assumption that there exist in the retina three different kinds of chromatic receptors. Is the evidence which has led to the abandonment of this assumption actually convincing? The experiments of Donders and others have shown that we can perceive colors of stimulus spots so small or distant as to form images upon single retinal cones. Moreover, the accepted opinion is that a point source of white light always looks white when clearly focussed on the retina, and does not tend to appear now red, now green and now blue, as would be expected if the centre of vision were made up of a mosaic of cones, individual elements of which were responsive singly to the red, green and blue constituents of the white light.[22]

As we have already noted, the recent researches of Hartridge have indicated very clearly the possibility of concentrating upon a single retinal cone a very large proportion of all of the light reaching the eye from a point source. However, the question certainly arises as to whether it is possible to maintain sufficiently constant fixation so that the light actually remains on any given cone during a period long enough to permit a judgment upon the stimulus effect which it produces. The actual effect upon consciousness, in other words, may not be due to the instantaneous action of the retinal image upon any single element but upon the summated effects of its successive actions upon a considerable number of adjacent elements. That a process of some such nature as this is involved in color perception, if not in brightness perception, is indicated by considerations advanced by Hartridge[23] with regard to the basis of discrimination between the colors of white and yellow points of light. The eye is subject to considerable chromatic aberration and in focusing an image upon the retina it selects the yellow rays of the spectrum as the ones to be most sharply defined. Any point source of white light would therefore be represented on the retina by a nucleus of yellow, which, according to Hartridge's calculations would fall upon a single cone, surrounded by a ring of blue which would fall upon many outlying cones. A point source of yellow light, on the other hand, would be represented on the retina simply by the yellow nucleus without the blue ring. Experiment shows that

[22] Cf., on these points, Parsons, *loc. cit.*, p. 120, and Schäfer, *loc. cit.*, p. 1112.
[23] *Loc. cit.*, pp. 198–215.

we easily distinguish the difference in color between two such small sources of light, but that we do not perceive the white spot to consist of a nucleus of yellow surrounded by a ring of blue. This means that the responses of a considerable number of cones have been combined to produce an integral sensation in consciousness.

This fusion in consciousness of the results of stimulating a number of adjacent cones may be attributable either to a fluctuation of the position of the image over the retina, due to a drift of fixation, or it may result from a general functional tendency for adjacent color excitations to fuse together. Such a tendency would not of necessity demand a similar fusion of brightness excitations. If we return to the original Helmholtz assumption, the brightness values would be represented by the strength of the nerve currents started at each retinal point while chromatic characteristics would depend upon the identity or location of the stimulated elements. These two entirely different kinds of data might be treated in quite different ways by the central nervous system. The limit of visual acuity for brightness patterns appears to coincide fairly closely with the dimensions of the individual retinal elements, indicating that there is very little fusion of brightness excitations between such elements. However, it is well known that acuity for patterns determined wholly by color difference, without accompanying brightness difference, is very much lower, and this indicates that fusion of adjacent chromatic excitations actually does occur.

It is improbable that, in our quest for an explanation of the transmission of the chromatic characterstics, it will avail us anything to fall back on the overworked neurofibrillæ. supposing that each optic nerve fibre contains a number of separate conductors which carry excitations back to the brain, in accordance with the "all or none" principle, independently of one another. Recent work throws grave doubt upon the actuality of these alleged internal structures of the nerve fibres.[24] They cannot be made out in the living nerve tissue by the use of the ultra-microscope, which renders visible other internal features of the cell. It seems very probable that these fibrillæ are coagulation products or "arte-

[24] Cf. Bayliss, W.: "The Principles of General Physiology," 1915, pp. 396–470.

facts" and are not actually present in the living cell, which is essentially a homogeneous liquid surrounded by a membrane.

Further experimental investigations of the questions discussed above would appear to be necessary in order to permit any reliable theoretical interpretation of the complex array of facts with which we have to deal.

VI. SUMMARY.

The present article discusses the relation to vision of the "all or none" principle in nerve action, on the basis of new experimental data. The general nature of the principle is first considered, together with the difficulties which appear when an attempt is made to apply it to the facts of visual response. An entoptic phenomenon—the blue arc effect—is then described, and evidence is presented that this phenomenon is attributable to secondary stimulation of certain retinal elements—probably the rods and cones—by the action currents of the retinal nerve fibres. Careful measurements of the luminosity values of this effect for a wide range of primary stimulus intensities show that it is practically independent of the latter, thus substantiating the view that the action currents of the retinal fibres really do follow the "all or none" principle. The conditions under which the measurements were made, and the methods of calculation, are described in detail. Representative spectral stimuli were employed, and the photometric values of the stimuli and of the effect, itself, for the various conditions of experimentation are presented in tabular form. In conclusion, some further theoretical implications of the results are considered.

THE COLORS PRODUCED BY EQUILIBRIUM
PHOTOPIC ADAPTATION

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THE COLORS PRODUCED BY EQUILIBRIUM PHOTOPIC ADAPTATION

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I. INTRODUCTION

In a recent paper Sheppard reports extensive and seemingly careful experiments which tend to support Hering's belief—a necessary deduction from his visual theory—that equilibrium adaptation of the retina to any stimulus yields a sensory quality of neutral mid-gray. Sheppard[1] finds that with practically all types of stimuli, whether high or low in intensity and whether or not of spectral purity, the final result of continued fixation is a gray. Sheppard's results seem to be in definite disagreement with the general outcome of quite an elaborate series of experiments which the writer made in 1914-15 at the Harvard Psychological Laboratory and which have not hitherto been described in detail. The experiments led to the conclusion that prolonged stimulation of the retinal cones with stimuli which initially evoke colors of considerable saturation does not in general bring about reduction of the sensation to a gray. In certain instances, the conditions of which can be quite definitely specified, such a reduction does occur, but the general rule was in sharp conflict with the implications of the Hering theory.

The experiments were reported verbally in summary at the meeting of the Experimental Psychologists at Harvard in 1917, and it is possible that Sheppard's work may have some indirect reference to the views which were advocated at that time. The results have also been mentioned in a general paper on "Apparent Brightness,'[2] incidentally in a paper on

[1] Sheppard, H., 'Foveal Adaptation to Color,' *Am. Journ. of Psychol.*, 1920, 31, 34–58.

[2] Troland, L. T., 'Apparent Brightness: Its Conditions and Properties,' *Trans. of the Illum. Eng. Soc.*, 1916, 11, 949–966.

special after-image phenomena,[1] and were utilized in 1914 to substantiate a general mathematical theory of adaptation[2] which was based on assumptions contrary to those of Hering. Since the question which is at issue is of great importance in determining the value of Hering's general conceptions for visual theory, as well as in relation to any supposed mechanism of visual excitation, it seems desirable that the results be reported in full. The writer believes that the discrepancy between his observations and those made by Sheppard and other supporters of the Hering theory will be found in some difference between the conditions under which the experiments were made, the principal factor being in all probability the control of the pupil. The majority of Sheppard's observations were made with the natural pupil, and those in which he employed an artificial pupil apparently did not involve any accurate control of the registration of the artificial pupil with the natural pupil. In all of the observations reported in this paper, on the other hand, or at least in those upon which reliance is placed for the refutation of the Hering theory, an artificial pupil was employed and care was taken to secure satisfactory register between the natural and artificial pupils. It is worthy of note that in Sheppard's series with spectral stimuli[3] in which an artificial pupil was employed he found it impossible to obtain 'complete adaptation' at high intensities.

The spontaneous fluctuations to which the natural pupil is almost always subject, produce variations in the illumination of the retina, and such variations result in very powerful after-image effects which are capable of quite submerging the normal course of the adaptation process. Only by carefully eliminating such fluctuations, and their accompanying changes in retinal illumination, can we hope to study the adaptation process in its purity. One of the most interesting features of the writer's work on foveal adaptation, only a portion of which will be reported in the present article, lay

[1] Troland, L. T., 'Preliminary Note: The Influence of Changes of Illumination upon After-Images,' *Amer. J. of Psychol.*, 1917, 28, 497–503.

[2] Troland, L. T., 'Adaptation and the Chemical Theory of Sensory Response,' *Amer. J. of Psychol.*, 1914, 25, 500–527.

[3] *Loc. cit.*, p. 55.

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Some of the general principles wh
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paper, and further details will be cons
and later articles.

II. The Quality of the Equilib

The majority of theories of visual
the intensity of any given component of
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With the possible exception of the se
function is such that when the velocity
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also such that an increase or decrease in
teristically accompanied by an increas(
tively, in the intensity. Since it is cu
chemical velocities as rates of change in
the reacting substance, or substances, w(
visual intensity considered is v, while th
substance is s, and t the time, these theo
that:

(1) $$v = f^d(\pm\, ds/dt),$$

where $f^d(\;)$ is a function having the prop

The above general premise of all
vision has obvious, important consequ

[1] By "the intensity of a sensory quale" is meant the
the degree of redness or of brightness.

the inevitable law that the velocity of a chemical change decreases or increases with a corresponding alteration in the concentration of the active substances, and such a change in concentration is the very thing called for by the rate of change itself. In general, then, without undue exactness of expression:

$$(2) \qquad ds/dt = -f^d(s)$$

and

$$(3) \qquad s = \int (ds/dt)dt,$$

that is, other things equal, the velocity is greater the greater the concentration of the reacting substance,[1] and the concentration is greater the longer the time during which the change has been occurring and the higher the velocity during that time. If we limit ourselves to a so-called irreversible monomolecular reaction, equation (2) becomes a proportionality, so that

$$(4) \qquad ds/dt = -ks,$$

where k is a constant. This equation states that the rate at which the substance, S, is being destroyed is at every instant proportional to its concentration. A simple integration of (4) gives us the relationship:

$$(5) \qquad \log s = -kt, \quad \text{or} \quad s = e^{-kt},$$

where e is the base of the natural system of logarithms. From (4),

$$(6) \qquad ds/dt = -ke^{-kt},$$

which asserts that the rate of decomposition of S becomes less as time goes on, approaching zero 'asymptotically.' But, from (1) it follows that

$$(7) \qquad v = f^d(\mp ke^{-kt}),$$

which states that with time the sensation *quale*, V, becomes less and less intense, and after a sufficiently long period will be reduced below the threshold and hence, psychologically, to

[1] The minus sign in equation (2) is due to the convention of taking the rate of change as negative when it is destructive of the substance, S.

zero. This process obviously correspon
called visual or retinal 'fatigue,' and in
actually observed changes in v with
similar to that demanded by the above

However it is one of the principal
mental work which is to be described be
least in one respect, equation (7) is not
that it is not an essential characteristi
that continued activity should result i
exhaustion. Now all chemical theories
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tive or 'katabolic,' the repair is of co
'anabolic,' and the latter change alway
term opposite in sign to that of the for
assumed that the repair goes on during s
thereafter, and if this is the case it is obv
tration of the sensitive substance will
zero, no matter how long the excitatio
suppose the repair process to have a ve
the intensity of the visual *quale* at that

$$(8) \qquad v = f^d(\mp ke^{-kt} \pm$$

so that after a very long time:

$$(9) \qquad v = f^d(\pm r).$$

If r is a constant or becomes so after
time, equation (9) represents a steady
which will continue indefinitely. Such a
we shall call one of *sensory equilibrium.*

It is characteristic of the visual the
certain visual qualities are attributed not
but also to the anabolic phase of the me
stimulation process of one sensation is
another sensation, which is defined

[1] Hering, E., 'Zur Lehre vom Lichtsinne,' *Sitzb.*
2 Aufl., 1878, p. 70.

Moreover, except in the case of the 'black-white substance,' when the opposed velocities are arithmetically equal these paired qualities completely inhibit each other, so that the theory in question implies a complete absence of both of these antagonistic qualities under conditions of sensory equilibrium.[1] As is well known, the qualities thus opposed are red to green, and yellow to blue. It is a recognized inconsistency of Hering's theory that black and white, although given the same psycho-physical basis as the other visual qualities, do not antagonize, but instead fuse with each other.

In harmony with Hering's point of view and what is in all probability a correct psychological analysis, we shall consider any visual sensation as resolvable into a limited number of component intensities, *viz.*, different *degrees* of black, white, red, yellow, green and blue. For these qualities we shall employ the abbreviations: N (noir), W, R, Y, G, and B, respectively. For the corresponding degrees we may employ the lower case letters: n, w, r, y, g, and b. Everyday experience teaches that, ordinarily at least, R and G are mutually exclusive, as are also Y and B; but that with this restriction a visual sensation may exhibit any combination of the variables named, with a wide range of relative intensities.

Let us define as an *equilibrium sensation* any sensation which accompanies a process in the visual system which involves no concomitant alteration in the sensitivity of the latter. Such a sensation will remain when the process of fatigue or adaptation has reached its asymptotic limit.

Now, as we have stated, it is a consequence of Hering's theory that there is only one possible constitution for such a sensation, *viz.*, $N+W$, in which $w=n$, a mixture denominated 'mid-gray.' According to this view, it can make no difference whether the equilibrium sensation is determined by an external or an internal stimulus, or whether the original sensation was chromatic or achromatic. All sensory processes, if permitted to go on undisturbed, must finally meet a common fate.

The most obvious method for an attempt to refute this consequence of Hering's theory would seem to be to fixate

[1] Hering, E., 'Grundzüge der Lehre vom Lichtsinn,' 1905–1911.

different stimulus fields during long pe
whether a time can be reached when
resolve themselves into a mid-gray. In
report is made of the results of experime
the stimuli employed covering a fairly
tions. The abolition of all visual differ
of fixation amounts to a 'disappearan
and it is a fact so well established that it
the outset that such disappearances ofte
of the problems to be considered is that
stances which make such lapses possibl
argue for Hering's view. This is a fiel
which already a great deal of work h
which, nevertheless, the writer believes
new facts. The final outcome of the p
that the equilibrium sensation is a neutr
certain very special cases, and the ph
disappearance will be given explanation
this proposition.

III. The Equilibrium Sensation wi Stimulus Fields

The simplest case in which to attac
quality of equilibrium sensations would
the complete exclusion of light from the
eye. This cannot be called an absence
especially in such a view as Hering's, it
darkness as a definite visual stimulu
doctrines are correct, darkness, like all
conditions, is a stimulus only when it
results from the cutting off of light, but t
of light is accompanied by a lapse of the
neutral mid-gray. The question theref
empirical consideration as to whether the
the completely rested retina in the dar
mid-gray. Hering himself considers
length[1] and admits that the visual quali

[1] 'Zur Lehre vom Lichtsinne,' p. 89.

ences upon awakening in the morning in a closely shuttered room suggests black more than it does white, and hence might be thought not to be such a gray. Hering explains this appearance by saying that although we have had experience of practically perfect whites, we have never had one of absolute black and that, consequently, our conception of mid-gray is weighted in the direction of white. This makes a true mid-gray seem too dark.

The author's observations confirm the naïve impression that the rest idio-retinal sensation is a great deal blacker than an unsophisticated mid-gray, but his reasoning does not wholly substantiate Hering's attempted reconciliation of such observations with his theory. In the first place, it is doubtful whether we can come any closer to an experience of pure white than we can to one of pure black. What reason have we for believing that 'pitch dark' lies farther from utter blackness than 'dazzling white' does from a catholic whiteness? Hering's argument, to say the least, presupposes his own theory. If, as such hypotheses as those of Helmholtz would imply, black is a result of the absence of retinal excitation, it would seem easier to produce a pure black than a pure white, since, theoretically, the white process could increase indefinitely in intensity whereas the black process would find its natural limit with complete retinal fatigue, a conceivably attainable state of affairs. In accordance with Hering's hypothesis, on the other hand, no natural limit can exist for either n or w. Consequently it would appear necessary to estimate both blackness and whiteness on the basis of mid-gray as a standard, rather than *vice versa*. If this is what we actually do the empirical evidence would seem to be against Hering's doctrine of the median quality of the equilibrium sensation in the absence of external stimulus forces.

As will be emphasized more strongly later on, it is probable that there is a central process in vision—closely allied with the functions of perception and discrimination—which causes us to become 'mentally blind' to sensory qualities which are uniformly distributed in both space and time. This process is slow-acting, but it obeys a law similar to that governing Hering's equilibrium sensation: it makes it impossible for an

observer to 'tell' whether or not the monotonous quality is present, and hence favors judgments of neutrality with regard to all sensations which have persisted for a long time unchanged. If such a process is active in the limitation of perception and introspection, we should expect the idioretinal rest sensation to be judged as mid-gray rather than as black or dark gray. Thus, if central adaptation fails to take effect, surely there must be some adequate peripheral cause, such as marked and permanent depression of the retinal activity below that characteristic of mid-gray.

Suppose, now that in place of darkness we fill the eye with a bright white light, and continue such stimulation indefinitely. If Hering's theory is correct, no matter how brilliant the light the final sensation will be that of neutral mid-gray.

Experiment I. The Equilibrium Sensation for Direct Sunlight on White Paper

As a preliminary test of this point the following experiments were tried. A large sheet of white drawing paper directly illuminated by the early afternoon sun on a cloudless day was viewed at such a distance as to yield a uniform stimulation field of 60 horizontal and 50 vertical degrees. Fixation of this field, which had a brightness—calculated from direct flicker photometric measurements—of about 346,600 candles per square meter, was steadily continued for ten minutes, in each trial, with the following results.

At the outset the stimulus appeared dazzlingly bright, and during the first thirty seconds or so it showed a very rapid decrease in apparent luminosity, so that at the end of about one minute it seemed as if illuminated by, roughly, one twentieth of the actual light intensity. Whatever significance may be assigned to an estimate of this sort, it is certain that the gray sensation to which it refers was very much brighter in each case than a naïvely conceived mid-gray. Moreover after the first minute no further darkening could be observed, and during the ensuing nine minutes whatever alteration did occur was in the direction of a restoration of the original brightness rather than that of its continued decrease. This experiment was repeated a number of times on different days

with the same results. It would appear from the above observations that (1) prolonged exposure of the retina to large, very intense, achromatic stimuli does not involve a reduction of the sensation to neutral mid-gray, but rather to a *bluish* gray the luminosity of which depends upon that of the stimulus and is much greater than that of mid-gray; and (2) that the reduction in luminosity which does take place occurs very rapidly so that the sensation soon comes to a permanent equilibrium state characterized by the just mentioned quality.

Experiment II. The Equilibrium Sensation for Direct Sunlight on Large Colored Papers

The principle of the reduction of visual sensation, by adaptation, to neutral mid-gray applies in Hering's theory not only to black and white but to all of the color qualities as well. As already stated, this is a consequence of his doctrine of mutually antagonistic color pairs. To put the question to an empirical test, large sheets of colored paper (of the Hering and Milton Bradley series) were fixated in direct sunlight under conditions exactly similar to those obtaining for the white stimulus just considered. Table I. summarizes the results of these experiments.

It will be seen by reference to the table that only in the case of yellow and green was there total disappearance of the color quality which was characteristic of the primary field and, even here, the disappearance was not permanent. In all cases there was, of course, a marked decrease in saturation and brightness, and sometimes a change in the exact hue-constitution of the field, very soon after the exposure was commenced. This change did not continue, however, and the quality of the area quickly settled down to an equilibrium condition which—apart from some tendency to revert to the original hue—was steadily maintained thereafter. It should be noted that those cases in which the primary hue showed phases of complete disappearance, were with stimuli of very high intensity, both the Milton Bradley yellow and Hering green being papers of high reflecting power.

Table I

The Fatigue Changes in Sensation with Large (
Intensity

General Conditions: Size of stimulus fields: approxima
vertically; Time of each fixation period: 10 minu
Hering (Rothe) and Milton Bradley series were em
direct sunlight on cloudless March and April days, b
T. Monocular fixation.

Explanation of Tables: In the first column, below, are s
of the stimulus, together with the abbreviations,
whether the papers used belong to the Hering or
respectively. The second column given the calcu
candles per square meter of the brightness of eac
data for these calculations being obtained by flic
actually employed. The next three columns conta
of the sensation during three successive periods o
that introduced by the rapid initial change, the t
second an intermediate phase.

Stimulus	Intensity	Color Phases		
		Initial	Intermed.	Final
NR M-B.....	38,500	r	r	r
R Her.....	50,000	wr'	wr	$w'r$
YR Her.....	88,500	wbg	yg	wry
Y M-B.....	215,000	w	wgy	wgy
Y Her.....	130,000	gy	$g'y$	$g'y$
G Her.....	80,500	$wg'b'$	wgb	wgb
GY M-B.....	117,500	$b'w$	$b''g''w$	$b''g''w$
BG Her.....	31,000	$b'w$	$b'w$	$b'w$
B M-B.....	69,000	$w''b$	$w''b$	$w''b$
RB Her.....	37,500	$w''rb$	$w''rb$	$w''rb$

Method of Symbolism: The letters used in the above t
ponent intensities defined at the beginning of the
are intended to indicate a *weakness* of the primed c
Thus *gy* stands for a quality about equally green a
the yellow is considerably stronger than the green,
far stronger. The quality orange is naturally syn
rb. The component *n* (degree of darkness) is ne
used only when needed to indicate a lack of satur
to the given stimuli. Strictly speaking, of course,
of all sensations, whether chromatic or achromatic.

General Results: The table shows that in those ca
of the stimulus entirely disappears or becomes very wea
50,000, and frequently than 100,000 candles per squa
are weaker than 50,000 candles per square meter show
already familiar fact that *B* is the most stable of the c
in this table.

The following generalizations may safely be made from the above described experiments. (1) Prolonged exposure of the retina to large chromatic stimuli of high, but not maximal, intensity is not accompanied by the disappearance of the hues characteristic of those stimuli or, necessarily, by a reduction of the brightness value of the sensation to that of a neutral mid-gray. (2) With such stimuli the qualitative change which does occur occupies a comparatively short time at the beginning of an exposure, after the end of which time a qualitative equilibrium becomes established. (3) When the chromatic stimuli employed are of exceedingly high intensity, complete disappearance of the characteristic hues may ensue. These disappearances occur soon after the beginning of an exposure, but they do not necessarily involve the abolition of all chromatic quality, and never mean a reduction of the sensation to the achromatic level of neutral mid-gray.

The preliminary experiments just described suggest very strongly that the equilibrium sensation in vision is not a monotonous gray but, instead, varies widely with the nature of the stimulus in connection with which the equilibrium is established. Of course we cannot be certain at the outset just how long it should take to bring the visual system to equilibrium under given conditions, but there are reasons for believing that in general the time should not be very great. The above experiments show that the sensory changes which follow the onset of stimulation at first involve a quite rapid decline of the characteristic stimulus quality; but although the alteration in the sensation during the first 30 or 60 seconds of the fixation is very readily perceived, that which takes place during the second minute (say) is—with ordinary light intensities—so small as to be hardly perceptible. Now it is just this sort of change which is demanded by a curve of the type presented in our equations (7) and (8) which, as we have seen, must go with such a chemical theory of vision as Hering's. However, there is a further qualitative consequence of the applicability of this form of equation to the phenomena which is not in harmony with Hering's teachings. If the

changes in sensation during a given con
are at first large, but rapidly become si
occurring thereafter prior to the atta
complete equilibrium, will also be very
the initial changes. In other words, if
of the greater part of the change in se
towards equilibrium a long time is to b
supposed also that a long time will be re
tion in the *rate of change* of sensitivity tc
matical explanation of this is apparent
all of the derivatives of the function
with respect to t are equal to the functi
each other.

If follows from this that, neglecting
effect of the function $f^d(\ )$ upon the c
rapid initial decrease in v, followed by
indicates that the sensation reached in
period of fatigue is very close to the e
Observation shows that in the majority
mate equilibrium sensation thus obtai
being a neutral mid-gray. Moreove
fixation, as we should expect, brings a
alteration in the quality which is left af
utes of fatigue. As already noted, there
that, if anything, continued fixation ma
the direction of the sensitivity change
fatigue is limited not by an asymptote
and the actual law of the adaptation
functions either different from or in
equation (8). We shall return to thi
point in the discussion.

With regard to the interpretation of
refutation of Hering's assumptions reg
obtaining at visual equilibrium, severa
raised, some theoretical, some experime
to throw doubt upon the notion that
equilibrium were reached in the experim

In the first place it may be asserted that the form of the function, f^d ( ) in equations (7) and (8) is such as to magnify changes in $-k\ e^{-kt}$ for large values of the latter and to minify such changes for small values, so that the time for the attainment of approximate equilibrium may really be great, although the apparent changes in the sensation are larger during the earlier than during the later parts of the fixation period. In answer to this it may be said that there can hardly be any doubt that f^d ( ) is logarithmic in general form—as demanded by Fechner's law relationship between the physiological visual process and the sensation—and that, consequently, its effect will be just the reverse of that required to weaken our argument against Hering. Equal changes in $-k\ e^{-kt}$ will produce smaller variations in v for high than for low values of the function in question.

Another objection, based upon experiment, may be drawn from the fact that if colored glasses are worn for a relatively long period of time, or if one is shut up in a room illuminated by a single color, one eventually becomes quite blind to the color in question. The experiment of Maria Bokowa[1] is classical in this connection. However, as previously indicated, there are strong reasons for believing that, besides visual fatigue in the ordinary sense of the term, we have in such phenomena as these to deal with a type of 'higher,' central alteration in sensation, such as is involved in the study of so-called 'memory colors.' When we are placed under the practical necessity of discriminating between objects for purpose of action we perceptually neglect all absolutely common qualities of these objects. Such perceptual neglect, or central adaptation may result, with sufficiently long exposure to a single color, in a nervous set which will remain for some time after the color is removed. This would involve blindness to the quality in question. The existence of a positive central control over the visual sensations cannot be denied.

[1] Bokowa, M., 'Ein Verfahren künstliche Farbenblindheit hervorzubringen,' *Zsch. f. rat. Med.*, 1863, 17, 161 ff.

IV. The Influence of Possible Central Factors on the Perception of Chromatic Qualities at Equilibrium

That central processes are actually involved in the fading of constantly fixated uniform color fields of large area is strongly suggested by the following observations.

Experiment III. The Restoration of a Chromatic Quality, Faded Through Adaptation, by the Introduction of a Neutral Comparison Object

If, during the intermediate period of the fixation of any of the large colored papers in Experiment II., a small white or gray object—such as the dial of a stop-watch—is introduced into the field, the original color is perceived with suddenly increased distinctness. The alteration in the appearance of the field is very remarkable, and yet it does not seem to be a purely sensory change: it lacks the characteristic *quale* of an objective alteration in illumination conditions and feels more like the reëstablishment of a decadent perception. This effect occurs for all the colors employed in Experiment II., regardless of the stimulus intensity. When the comparison object is introduced the color field turns darker at the same time that its original quality is again clearly distinguished.

In these experiments the comparison object is of course suffused with a color complementary to that of the original field, and hence the rejuvenescence of the latter quality would naturally be attributed to simultaneous chromatic contrast. It is clear, however, that with regard to the retina at least, the situation must be somewhat different from the usual case in which the contrast-inducing color depends upon the specific quality of the stimulus. Moreover, in the effect which we are now considering the contrast-inducing object is small compared with the field which it affects, and yet its influence is very striking. Its law would therefore appear to be quantitatively, if not qualitatively, different from that of ordinary color contrast. That the two cases are really distinct is proven by the following experiment. A dark gray square of size approximately 9 degrees, was placed in the center of the

yellowish green Hering paper, and was fixated in direct sun light until the green component had entirely faded, leaving a light blue—a state of affairs which continued for four minutes. At the end of this time a shutter arrangement was released which substituted in place of the gray square an equal area of Hering violet paper. Under these conditions there was no reappearance of the green, the outlying field becoming simply a somewhat darker blue. Immediately, however, a white object was introduced into the green adapted area, the latter color returned.

Simple tests were made to determine whether the reappearance of the adapted color might not be due to a contraction of the pupil, owing perhaps to the correction of a relaxed accommodation when the comparison object was introduced. In these tests an object of about the same brightness as the large field was employed, and care was taken to keep the fibers of the paper clearly in focus throughout. This made no difference in the results. As will appear later, it is probable that contraction of the pupil would tend to bring out the complementary quality rather than that proper to the field, although it is not entirely certain that this would be the case for the high degrees of illumination here employed.

The above experiments, while they support the view that color sensation is subject to central control, also indicate that 'central adaptation,' if it be a fact, can function as a local process, and hence is not strictly equivalent to 'forgetting what the color looks like.'

Experiment IV. The Equilibrium Sensation for Large Contrasting Colored Paper Fields

In order to study the importance of contrast and discrimination processes for the changes occuring in large fixated fields of color, observations under conditions similar to those of Experiment I., were made; the stimuli consisting of two juxtaposed Hering papers of different hue, the one filling the upper half of the field and the other the lower half. The six possible combinations of the four *Urfarben* were employed and the results are shown in Table II

An examination of the table in question will reveal the presence of a rather well marked mutual influence of the contrasting colors of the stimulus in the determination of the equilibrium qualities. This interaction appears to follow the law of ordinary complementary induction, as would be expected. For example, the red with the green loses less of saturation than it does with the yellow, while with the blue it becomes orange instead of pink. The green with the blue becomes yellowish, whereas with the yellow it turns toward the blue. The cases in which this complementary influence is smallest appear to be with the blue and with the yellow. This fact may be attributed to the much greater brightness of the yellow and greater darkness of the blue, as compared with the other two colors, or else to the well-known superior stability of the *Y* and *B* components, in general. The results of the present experiment would appear to support the conclusion that the principle of simultaneous color contrast applies to the equilibrium condition of the sensation, as well as to other phases. Whether this fact is to be attributed to central or to peripheral action is a question which must be considered an integral part of the still unanswered query as to the meaning of contrast, at large. From the writer's present point of view it seems to him probable that complementary color contrast in general is a central process, being one case of the sort of regulative activity of the centers with regard to the sensation quality already suggested above.

It must not be inferred from what has just been said that the results summarized in Table II. represent a contrast effect of the same order of magnitude as that considered in Experiment II. On the contrary, the introduction of a neutral object into the bi-chromatic field brings about the same striking change in quality, for both halves of the field, which occurs in the case of the simpler stimulus.

Observations under conditions exactly similar to those specified in Table II. were carried out with bipartite fixation fields composed of black and white—black velvet and white drawing paper—respectively. The intensity of the former was about 1,250 candles per square meter, and of the latter

Table II

THE FATIGUE CHANGES IN SENSATION WITH LARGE BICHROMATIC STIMULUS FIELDS OF HIGH INTENSITY

General Conditions: Size of stimulus fields: approximately 50° by 50°, the upper half being filled by one color and the lower by the other; Time of each fixation of the middle of the line of demarcation: 10 minutes. The papers were from the Hering series only. These were viewed in direct sunlight on cloudless March and April days, between 1 and 3 P.M. Subject: *T.* Monocular fixation.

Explanation of Tables: The arrangement of the table, and the symbolism employed is the same as for Table I., above. Brackets are used to indicate the pairs of stimuli fixated together in the double fields.

Stimulus	Intensity	Color Phase			Remarks
		Initial	Intermed.	Final	
{ *Y*.....	130,000	*y''g''w*	*ygw*	*ygw*	There was a short initial phase
{ *R*.....	50,000	*r''w*	*rw*	*rw*	of nearly equal luminosity.
{ *R*.....	50,000	*r'w*	*rw'*	*rw'*	
{ *G*.....	80,500	*w*	*g''b''w*	*g''b''w*	
{ *R*.....	50,000	*ryw*	*ryw*	*ryw*	
{ *B*.....	6,700	*bw*	*bw*	*bw*	
{ *Y*.....	130,000	*gy*	*g'y*	*g'y*	Same effect as for *Y* and *R*,
{ *G*.....	80,500	*b'w*	*b'w*	*b'w*	above.
{ *B*.....	6,700	*bw*	*bw*	*bw*	
{ *Y*.....	130,000	*gy*	*gy*	*gy*	
{ *B*.....	6,700	*bw*	*bw*	*bw*	
{ *G*.....	80,500	*w*	*yw*	*yw*	

approximately 250,000. At no time during the ten-minute fixation period was there even a tendency for the two halves to fuse in a common neutral mid-gray, as demanded by Hering's theory. The white became greatly reduced in apparent luminosity and assumed a bluish tinge during the first minute, but after that suffered no change, except possibly a slight increase in brightness. The black showed no change at all. Experiments were also made with a piece of white paper bearing a diffuse smudge of smoke—a pattern recommending by Hering for the demonstration of the lapse of visual differentiæ through fatigue[1]—which was fixated in bright sunlight. Momentary disappearances of the brown smoke spot were obtained, but these were always during the first minute or two after fixation. Even at the outset the

[1] Hering, E., 'Zur Lehre vom Lichtsinne,' *Sitzb. d. Wien. Akad.*, 1872–1874, 2 Aufl., 1878, p. 96.

smudge was a fairly bright gray, a fact which helps to account for its easy disappearance.

In all of the experiments thus far described the writer acted as subject.

The experiments described in the present Section offer a certain degree of confirmation for the idea suggested at the end of Section I. that the disappearance or weakening of some of the components of visual sensation may have central as well as peripheral causes, a procedure being pointed out whereby the original qualities can be restored without resting the retina. The success of this procedure appears to rest upon a central principle of color contrast. In line with this it is shown that the law of complementary color induction applies to the equilibrium, as to other, visual sensations.

V. The Fluctuation in Visibility of Spot Stimuli on a Dark Ground, and Related Phenomena

Suppose, now, that we pass from the consideration of the probable equilibrium conditions which go with large intensive stimuli of uniform quality to those obtaining with simple patterns in which the illumination is less intense. The simplest case is that of a small round luminous spot on a dark ground. A large number of investigations are already on record with regard to the course of the sensation conditioned by such stimuli, with constant fixation. It is a well-known fact that when the illumination of the spots is at low intensity they readily disappear, and either remain below the threshold or fluctuate between visibility and invisibility.[1] If the dark outlying field could be considered as a neutral mid-gray, this would present a case of the complete reduction of a sensation by adaptation. These disappearances also occur when the intensity of the stimulus is moderately high, but they are less frequent and last a much shorter time than with the dim lights. The general law appears to be that the phase of visibility increases at the expense of that of invisibility with increasing intensity.[2] For very low intensities disappearance may be

[1] Ladd, G. T., 'A Color Illusion,' *Yale Studies*, 1898, 6, 1 ff.

[2] Marbe, K., 'Die Schwankungen der Gesichtsempfindungen,' *Phil. Stud.*, 1893, 8, 615–637.

permanent, if the spot is not too small, but for very high ones it never occurs in the first place.

These fluctuations of visual sensation under the influence of a constant external stimulus receive ready explanation in terms of the Hering theory, if it is supposed that slips of fixation occur, during which a rapid change in the state of adaptation of the visual system can take place. If, prior to the eye-movement, the visual cells were at equilibrium with the stimulus, the field of sensation must have been a neutral mid-gray, but as soon as the ocular displacement occurs, differential recovery sets in so that upon a return to the original fixation the spot is again momentarily seen. Ferree[1] has reported experiments in support of this interpretation of the fluctuations. The Hering theory of the mid-neutrality of the equilibrium sensation, however, does not account for the relation of the relative lengths of the phases of visibility and invisibility to the intensity of the stimulus, although it is directly in harmony with the fact, adduced by Ferree, that for certain low intensities very small areas show no disappearances while very large ones disappear permanently.[2] The imperfectness of fixation always suffices constantly to present fresh retinal surfaces to a very small stimulus spot. For complete harmony with the theory, the rule of disappearance, without return, of large areas should hold for all intensities.

The fluctuations in the gray bands presented by the classical Masson's disk have, for a long time, been attributed to momentary 'lapses of attention,'[3] in spite of the obvious fact that under the usual conditions of the experiment, attention to the disk is probably maximal at just the moments of disappearance of the bands. Heinrich and Chwistek,[4] and others, have sought the cause of the fluctuations in spon-

[1] Ferree, C. E., 'An Experimental Examination of the Phenomena Usually Attributed to Fluctuation of Attention,' *Amer. J. of Psychol.*, 1906, 17, 81–120.

[2] Ferree, C. E., 'The Intermittence of Minimal Visual Sensations,' *Amer. J. of Psychol.*, 1908, 19, 59–129.

[3] Lange, N., 'Beiträge zur Theorie der sinnlichen Aufmerksamkeit und der activen Apperception,' *Phil. Stud.*, 1888, 4, 390–422.

[4] Heinrich, W. und Chwistek, L., 'Ueber das periodische Verschwinden kleiner Punkte,' *Zsch. f. Sinnesphysiol.*, 1906, 41, [illegible] 74.

neutral mid-gray, and if its brightness is in some sense proportional to the intensity of the stimulus, the disappearances might be expected to be more frequent for the lower than for the higher intensities. With the 2° white stimulus at an intensity still higher than in either of the cases considered above, subjects *H* and *E* failed to observe any disappearances during continuous fixation periods of 30 minutes. Similar results were obtained with subject *E* when the spot was viewed for 30 minutes through a ruby red glass. At higher intensities the sensation from this latter stimulus was readily reduced to a pure yellow. A large number of experiments were carried out with a very bright red stimulus—containing only the orange and red of the spectrum—of visual angle approximating 6.4°, in which the fixation periods were varied from 7 to 10 minutes. Although the subjects (*E*, *H*, and *T*) reported apparent sensory equilibrium within 30 seconds to 4 minutes no cases of disappearance were observed. With a somewhat weaker light there was no disappearance even of the *R* component of the sensation. Similar results were obtained with a green stimulus of the same dimensions, except that no instance of complete disappearance of the green component was reported in the few trials made with this color.

VI. The Influence of the Size of the Pupil on the Visibility of Stimuli

The theory of Hering would explain the fluctuative disappearances of spot stimuli by complete reduction, through adaptation, of the original sensation to a neutral mid-gray. The fluctuations would then be regarded as *reappearances* due to eye-movement[1] or other external causes, which permit momentary phases of reversal of the adaptation (recovery). However, the force of this explanation obviously is gone if the conception which we have been attempting to support in the foregoing general argument be admitted, *viz.*, that equilibrium adaptation does not bring all sensations to a common level regardless of the nature of the stimuli which

[1] Ferree, C. E., 'An Experimental Examination of the Phenomena Usually Attributed to Fluctuation of Attention,' *Amer. J. of Psychol.*, 1906, 17, p. 83 ff.

more, no *permanent* disappearance could be obtained, the actually observed obscurations being very brief. The stimulation was monocular, the other eye being heavily blindfolded.

Fluctuations of this sort obviously cannot be explained on the basis of complete fatigue followed by recovery due to eye-movement, because, under the conditions, deviations in fixation would not move the image on the retina through much more than one fiftieth of its total diameter. Changes in accommodation, also, could exert no influence on the course of the sensation, since with large uniform stimulus surfaces they could produce only edge effects upon the image. There exists, however, a very potent factor in the limitation of the action of an external stimulus upon the retina which, for some unexplained reason, has received comparatively little attention in general visual experimentation, namely, *the size and activity of the pupil.*

The diameter of the average human pupil is capable of variation from a maximum of 7.5 mm. to a minimum of 1.5 mm., although the usual aperture lies between 2 and 6 mm. Since the amount of light entering the pupil varies as the square of its diameter the intensity of a retinal image which, with a maximal pupil is 1, will with a minimal pupil be 1/25, *i.e.*, with the former an external stimulus intensity of one candle per square meter is visually equivalent to a stimulus intensity of twenty-five candles per square meter with the latter. Now although the size of the pupil is for the practical purposes of seeing a function of the external illumination, this size is by no means invariant during steady stimulation, oscillations as great as 2 mm. being quite common. All changes in the size of the pupil which occur in the presence of such stimulation must of necessity have an effect upon vision which is identical with that to be produced by actual variation in the stimulus intensity. That such variations are ideally fitted to cause fluctuative disappearances of the type we have just been discussing, a moment's consideration will suffice to show.

cidences between pupillary contractions and the disappearance signals were noted. Practically every strong contraction was followed immediately by a signal. The best conditions for observation appeared to exist when the activity of the pupil became rhythmic. As a sample of the results obtained, the following trial with subject *D* may be described. At the beginning of the fixation the pupil had a fairly large opening, about 4 to 5 mm., and showed some fluctuations rather difficult to follow. During the preliminary period the subject gave some disappearance signals for which a clear correlation with pupillary action could not be affirmed. After the lapse of an interval which would seem to correspond roughly with the time required for the retina to reach sensory equilibrium, the pupil began rather suddenly to show sharp rhythmic contractions, in which the diameter changed from about 4 to about 2 mm. During a continuous series of twenty of these contractions there was only one for which the subject did not give a signal at an interval of 2/5 to 3/5 of a second after its completion, a period which corresponds very closely to that required to produce the dimming effect. The subject reported each time the (Hering red) color wholly disappeared from the field of vision. After a rest the observations were resumed, with exactly similar results in every respect. These two series then give 38 cases of perfect coincidence between pupillary contraction and disappearance of the stimulus, out of 40 cases of observed contraction, and during the periods of easily observable contractions there were no cases of disappearance which were unaccompanied by the pupillary change. The subject stated that the disappearances which occurred in the preliminary periods of small fluctuations were not as well marked as those which followed.

There appears to be evidence in these observations of a definite rhythmic reflex of the pupil, the biological significance of which may perhaps lie in the possibility of producing clear intermittent vision where satisfactory continuous vision is impossible owing to adaptation. The contraction of the pupil will have an effect similar to that attributed to eye movement in the case of small luminous stimuli, and permit the retina

to the report of disappearances when no contractions were observed, it may be said in the first place that, except where the changes in pupillary size have a considerable amplitude, they are not easy to record by the method of observation here employed, yet states of adaptation may occur where a small contraction will suffice to cause a disappearance. Contractions which start with a wide pupil, if themselves small, do not alter the intensity of the retinal stimulus by a large fraction, but they may nevertheless be effective in producing disappearances, owing to the fact that they act upon the basis of a strong prior 'fatigue.' On the other hand, linearly small contractions which start with a narrow pupil cause a relatively large change in the illumination of the retina.

It is a well-known fact that, in the so-called accommodation reflex, the pupil undergoes contraction simultaneously with the adjustment of the lens of the eye for near vision. Consequently the observations reported by other investigators which go to show that changes in accommodation coincide with phases of disappearance of visual stimuli, may receive an interpretation in terms of the activity of the iris. For stimuli of very small area, any accommodation change which causes blurring may produce a dimming effect by spreading the light out over a larger area. However, the probable efficacy of this change in the production of disappearances is doubtful, on account of the fact that this same process of spreading brings the light to fresh retinal surfaces, and it is a well-known fact that for small areas the threshold depends upon the total light energy received rather than upon the illumination. The effect would be limited in any case to very small stimuli.

In their crude outlines the above reported experiments do not prove that the stimuli which were observed to fluctuate were actually of supraliminal sensory equilibrium, since it is obvious that a stimulus which disappears through simple fatigue may be brought once more to visibility through a dilation of the pupil—the effect of which upon the retina is the same as brightening the stimulus. This will be true especially if the dilation is preceded by a contraction. How-

these observers reported fluctuations. However, their methods of work were not such as to yield conclusive results with regard to that sort of influence of the pupil in which we are now interested, McDougall's primary interest being in retinal rivalry phenomena. Consequently further work along these lines would seem to be desirable. The employment of the artificial pupil provides a better means for the elimination of pupillary effects from the adaptation process in vision than do the drugs, atropin and pilocarpin. Such a pupil, of diameter 2.36 mm., was used by the writer in all of the measurements to be described below. In the course of these measurements several thousand observations of spot stimuli of medium brightness—ranging in size from .7° to 3.5°—on a dark ground, with fixation periods varying from one to 15 minutes, were made, and the number of fluctuative disappearances which were seen could be counted on the fingers of one hand. This would appear to be conclusive proof that the variations in size of the natural pupil are the essential cause of these disappearances, when the pupil is free to modify the intensity of the retinal stimulus.

The above statement applies only to stimuli of moderate or of high intensity, and not to those of very low intensity. If the latter are what we have designated as 'of subliminal sensory equilibrium' it is obvious that adaptation will cause their disappearance, and that when their equilibrium sensations lie very close to the threshold,[1] slight disturbances, such as slips of fixation, changes in the nutrition of the retina, etc., will cause them to fluctuate in visibility.

Experiment VIII. Measurement of the Minimal Stimulus Intensity Necessary to Prevent Fluctuative Disappearances with the Artificial Pupil

In order to determine the intensity of such an equilibrium threshold stimulus the following experiments were made. The results apply, of course, only to the special conditions under which the measurements were taken, *viz.*, for foveal vision with a dark contrast field.

[1] The threshold concept throughout this paper is clearly differential as well as absolute.

In the case of the red and yellow the stimulus retained its original color, in good saturation, at the intensity in question. With the green and blue, however, there was a strong tendency for it to pass over into a gray before disappearing. This well-known phenomenon of a photochromatic interval in the case of lights of the shorter wave-lengths, made it necessary to modify the experimental method for stimuli of this sort so that the threshold intensity value finally found was one for which the *color* remained constantly visible, although a considerably lower value might have sufficed to keep the spot in view as a gray. The photochromatic interval is usually supposed to be absent with strictly foveal stimuli,[1] and the few observations here reported cannot be regarded as adequate to refute this view, since slight slips of fixation easily bring the stimulus into the extra foveal region when it has fallen below the threshold in the fovea itself.

The observations and measurements just described show that if the action of the iris is eliminated, a stimulus of low intensity is able to maintain a steady sensation without loss of its proper quality for long intervals. The periods of time employed are probably sufficiently protracted to justify the assumption that the measurements apply to the equilibrium sensations of the stimuli which were used. These results, then, argue very strongly against the Hering view that equilibrium sensations are a uniform neutral gray, and in combination with facts previously considered constitute proof of the efficacy of the natural pupil as a cause of visual fluctuations.

The fluctuations which are actually observed near the threshold are probably somewhat complex in their causation. Some of them are no doubt to be explained on the eye-movement-recovery theory, either in its ordinary form or with respect to the influence of these movements on the amount of light entering the eye through the artificial pupil. Large deviations from 'register' (co-centering) of the natural and artificial pupils cut off some of the light and this produces a dimming effect. Registration was secured at the beginning

[1] Nagel, W., 'Adaptation, Dämmerungssehen und Duplizitätstheorie,' *Helmholtz's Physiol. Opt.*, 3 Aufl., 1911, 2, p. 309.

seem to him correct, since by far the greater number of disappearances of such color fields involve a submergence of the spot in one of these bright surgings. Another cause is probably binocular rivalry with the opposite wholly dark field. It is clear that the above measurements of the equilibrium threshold, which indicate an average value of about 1.2 candles per square meter with a pupil of 2.36 mm. are not minimal, because they indicate a factor over and above that required by considerations of pure adaptation, which is necessary to counterbalance the disturbing influences just mentioned. Moreover, it would be absurd to claim that, with prolonged fixation, cases might not arise in which this factor would have to be increased considerably.

Experiment IX. The Equilibrium Sensation for Spot Spectral Stimuli of Equal Brightness, with the Artificial Pupil

The experiments just described were extended in a series of observations made by subject *B* in which spectral stimuli of angular size 1.28°, only, were used, with fixation conditions similar to those in the first series. Eight lights were employed which were equated in respect of brightness by means of the flicker photometer, the common intensity being 44.7 candles per square meter, an artificial pupil of 2.36 mm. being utilized, yielding a photon value of 195.1. The ranges of wavelengths employed were the four used above, together with a violet, blue-green, yellow-green, and orange. The flicker equations were made by subject *L* for another purpose, and were not verified for subject *B*, because of limitations of time and the fact that both were thoroughly normal in their color vision, subject *L* having himself done extensive work upon color. The results of the ten-minute monocular fixation of these stimuli are given in Table IV.

The primary object of this series of observations was to determine whether spot stimuli of moderate intensity show radical changes in apparent quality at sensory equilibrium. The results indicate in the first place that with the artificial pupil no disappearances of such stimuli occur, and that even

adaptation to large fields of stimulation, *i.e.*, that not only is the maximum depression of the sensation reached quickly (in one to three minutes), but that after this time there is a revival of the original quality.

During the course of the work with the method of critical flicker frequency to be described in a second paper, a considerable number of long interval fixations of the same spectral colors used above were carried out. The light in these cases was interrupted at the rate of 25 alternations, of equal light and dark periods, per second, the brightness of the stimulus being, during the greater part of the time, very close to that required to eliminate flicker at this frequency, approximately 30 candles per square meter. The fixation periods were nine minutes or more, subject *T* being the observer. The dimensions of the stimulus spot were the same as specified for the series just described. At least four cases with each color, two for each eye, were recorded. The results were that the only stimuli completely to lose any of their characteristic components were the violet and the blue-green, which lost their *R* and *G*, respectively, both becoming a whitish, unsaturated blue. All of the colors of course became less saturated and darker. The *G* component did not disappear from the yellow-green or the *R* from the orange. The red became slightly bluish instead of yellowish. The longest cases of fixation were of the red for 30 minutes, of the green for 21, and of the blue for 15. It is worth noting that practically no after-images were visible at the end of these long fixation periods, when the stimulus was removed and the eye left in the dark. This latter observation was repeatedly verified by subject *H*.

Experiments were not made with spot stimuli of high intensity, since the phenomena which they exhibit are quite well known. 'Fatigue' and increased intensity coöperate to extinguish, first, the *R* and *G* components in the sensation and then all chromatic elements, so that the stimulus appears white. This occurs with pure spectral lights at sufficiently high intensity, as well as with heterogeneous chromatic stimuli.

the original brightness and the time of preëxposure. In all cases the spot reappeared almost immediately, and remained visible thereafter as long as fixation was continued. In the case of the blue the reappearance was often that of a gray rather than a blue spot. The quantitative measurements made upon these phenomena were insufficient to permit any very secure generalizations, but they strongly suggest the view that regardless of the degree to which a specific sensitivity of the visual cells has been reduced by prior stimulation, this specific sensitivity will automatically readjust itself to a succeeding stimulation, with an end result which is practically independent of the preliminary 'fatigue.' The experiment proves, in general, that Hering's doctrine of the neutral median character of all equilibrium sensations cannot be true, since in these observations the movement towards equilibrium may be away from the neutral mid-gray and in the direction of the quality proper to the stimulus.

Objections to the above conclusions, based on the notion that recovery here may have taken place during an eye-movement incident upon the subjective disappearance of the stimulus spot, cannot be admitted, since lapses of fixation were easily guarded against by means of the bright crescents which they yielded on the edges of the fatigued area. The process of recrudescence of the sensation could be clearly observed to occur within the border defined by these crescents. With care, a practiced observer can maintain his fixation of a circular spot of the size here employed, to within about one sixteenth of its diameter.

VIII. The Influence of Binocular Rivalry upon the Visibility of Monocularly Viewed Stimuli

The work with the artificial pupil which has been described above shows that the liability of specific visual qualities to suffer extinction through processes of adaptation is much less than is commonly supposed. The experiments indicate very clearly the preëminent importance of pupillary fluctuations in bringing about the much-discussed fluctuative disappearances of visual areas. However, it would be folly to claim

quality. The white was described as very brilliant and possessed of the characteristics common to binocular luster, so that the idea is clearly suggested that it was the result of a central combination of the after-image process of one eye with the primary image of the other, to form a complementary white. Time did not permit a careful investigation of this phenomenon to determine whether or not it could be obtained with blue and violet under the right conditions.

In the course of the work with large colored stimuli described in Experiments I. and II., it was found by subject *T* that if, after prolonged fixation with the right eye, the left eye was exposed to the stimulus and the right blindfolded, strong periodic darkenings, characterized by a pebbly pattern and a tinge of the hue complementary to the stimulus, appeared in the field. These darkenings were very patchy—and seemed generally to omit the foveal region—but where they occurred they entirely obscured the stimulus. If after a short fixation period with the left eye, the observation was continued once more with the right, similar surgings of the field were observed, except that in this case they were usually not darkenings, but consisted of strong patches of a color different from that of the stimulus. As in the case of the left eye the patches entirely supplanted the proper quality of the stimulus, except when—following the procedure of Experiment III.—a neutral comparison object was introduced, which permitted the original quality of the stimulus to be perceived through a mist of the opposing quality. For the orange, yellow, yellow-green, and blue-green papers this latter quality was purple. The red paper showed bright yellow patches, the blue simply darkenings, the purple bright green blotches, and the violet a general whitening of the field. In the case of the green, the purple color persisted during two minutes' continuous fixation in sunlight. In all of these observations only one eye was externally stimulated at one time.

It would be foreign to the purpose of the present paper to attempt a theory of the phenomena just described. Their sufficient significance in the present connection lies in the proof which they offer of the fact that, under certain con-

its magnitude will be proportional at all times to the concentration of the substance at the time. If we assume the reaction to be irreversible, the spontaneous decomposition in question may be represented by the equation:

$$k = c_k s, \tag{10}$$

where c_k is a constant. Similarly, the component of the rate due to the action of light may be represented by the equation:

$$d = c_d i s \tag{11}$$

in which i is the light intensity. In order to maintain the process, repair is necessary, and if this repair consists of a flow of substance into the cell which is governed by osmotic principles, it should follow an equation similar to:

$$a = r - c_r s, \tag{12}$$

where a is the rate of inflow, c_r a constant, and r another constant, whose magnitude depends upon the concentration of the nutritive substance in the lymph outside the cell.

It will be clear from a consideration of the above equations that, on the basis which they provide, the total rate of change of s, which latter variable is equivalent to the sensitivity of the cell, must be

$$ds/dt = a - k - d \tag{13}$$

or

$$ds/dt = r - (c_r + c_k + c_d i)s. \tag{13a}$$

But at equilibrium $ds/dt = 0$, for which value equation (13a) can be reduced to:

$$s = r/(c_r + c_k + c_d i), \tag{14}$$

which states that the more intensive the stimulus with which the visual cell comes into equilibrium, the lower will be its equilibrium sensitivity. Now, we have assumed the intensity of the sensation to be proportional, in some sense, to the katabolic component of the change in s. This component obviously has the value:

$$q = (c_k + c_d i)s, \tag{15}$$

or, for equilibrium, from (14):

$$q = r(c_k + c_d i)/(c_r + c_k + c_d i), \tag{16}$$

of time during which such an object can be continuously perceived, seems to the writer, on the basis of the work described above, very doubtful.

Ferree[1] has attributed certain visual fluctuations to the effects of 'the streaming phenomenon,' or the surgings of the idio-retinal light. As already noted, the present writer's observations tend to bear out this view with regard to liminal stimuli seen with the artificial pupil. When the idio-retinal field is quiet there are far less marked fluctuations in the visibility of the liminal stimulus, and the intensity which is necessary in order to maintain it constantly in view is lower than when strong surgings of the idio-retinal light are present. The blotting out of spot stimuli by the idio-retinal surgings appears to be a matter rather of swamping than of inhibition. If the intensity of the stimulus is increased so that the sensation which it causes is always perceptibly greater than the brightest of the idio-retinal waves, it shows no disappearances. Accordingly it seems probable that disappearances due to the streaming phenomenon should be attributed to momentary increases in the value of the second complex term in equation (18), which causes the difference between the two terms to become less than Δq.

Momentary disappearances of a visual stimulus may sometimes be due to rapid eye-movements which, as Holt[2] has shown, are accompanied by central anaesthesia. Even if there were no such anaesthesia, the sensation might lapse, in the case of minimal stimuli at least, owing to the speed of the movement and the consequent lowering of the time of action of the stimulus upon a given retinal point. In such cases the stimulus would reappear again almost immediately, as fixation was reëstablished. Some of the observed disappearances of very dim lights are so nearly instantaneous as strongly to suggest an explanation of this sort.

The reappearance of stimuli of subliminal sensory equilibrium as a result of recovery during eye-movement is also,

[1] Ferree, C. E., 'The Streaming Phenomenon,' *Amer. J. of Psychol.*, 1908, 19, 484–503.

[2] Holt, E. B., 'Eye-Movement and Central Anæsthesia,' Psychol. Rev. Monogr. Suppl. No. 17. *Harvard Psychol. Stud.*, 1903, 1, 3–46.

It is probable, also, that at high intensities the relation of antagonism enters in, since if each of the components approaches an asymptotic limit with increasing intensity, the same values will be attained by antagonistic excitations at high intensities of any stimulus, regardless of the wave-lengths which it includes, and it is naturally to be assumed that at such limits the antagonists, being probably at about equal strength, will either inhibit each other or combine to form white. In either case the two antagonistic qualities will be eliminated as possibilities in sensation. For very low intensities it is probable that, through dark adaptation, a marked increase in the sensitivity of the achromatic mechanism, relative to that of the chromatic, may cause a disappearance of color components through 'swamping.' This is probably true in the case of the achromatic interval in the blue, since at both very low and very high intensities the blue of the spectrum yields a sensation having a strong W component.

All of the above considerations apply to observations with the artificial pupil as well as to those in which the natural pupil is free to modify the effective intensity of the stimulus. In the latter case, however, the writer is convinced that with stimuli of ordinary intensity, practically all of the observed fluctuative disappearances are due to the activity of the iris. Several types of fluctuation caused by the pupil may be suggested. The first applies to a subliminal equilibrium stimulus. We will suppose that the sensation has reached equilibrium with a fairly wide pupil and that it has fallen below the threshold. If the pupil now contracts, recovery will set in, that is, the level of sensitivity will be raised in accordance with equation (14), owing to the weakness of the effective stimulus with which the organ is in equilibrium. When the pupil expands once more the stimulus momentarily reappears. It is clear that whether a given external stimulus is one of subliminal or of supraliminal equilibrium may be determined by the size of the pupil itself, since this affects the energy reaching the retina just as does an actual variation in the external illumination. Hence a second case may be suggested in which a subliminal equilibrium is reached with a

TEMPERAMENTAL DIFFERENCES BETWEEN OUTBRED AND INBRED STRAINS OF THE ALBINO RAT

NENOZO UTSURIKAWA

From the Harvard Psychological Laboratory

INTRODUCTION

About two years ago the writer sought, in the Harvard Psychological Laboratory, training in the methods of comparative psychology, since such training promised to be helpful to him as an ethnologist. A problem was suggested to him by Professor R. M. Yerkes,—evidently difficult and yet extremely fascinating. Its thorough study would certainly require years of diligent work. But the writer, because of his ethnological interests, was able to give only one year to this psychological investigation.

Obviously enough, from what follows, the materials to be presented are fragmentary and inadequate for the description of the differences in the strains of rat. Still, to throw them away would seem too extravagant. With a humble sense of obligation, the writer offers his limited data to the scientific world. He wishes to take this opportunity to thank Professor Yerkes, Dr. R. M. Elliott, and Dr. W. R. Miles, for valuable assistance in the work.

PROBLEMS

The chief problem was to discover, if any, the temperamental differences between outbred and inbred strains of the albino rat. Such features of behavior as degree of nervousness or timidity, of savageness and wildness, of sensitiveness to stimuli, of persistence in response, quickness of response, and so on, may be recorded as constituting the temperament of an animal. In the terms of psychology, and in the last analysis, perhaps, temperament is identical with the threshhold, quickness, amount, and steadiness of response to a given stimulus or object. The

OUTBRED STOCK—*Continued*

Number of rat	Source and parentage	Date of birth	Experiment begun
1 ♂	Wistar	September —, 1914	November 5, 1914
2 ♀	"	"	"
3 ♂	"	"	"
4 ♀	"	"	"
261 ♂	Granby, 251 ♂ x 252 ♀	October 28, 1914.	March 2, 1915
262 ♀	" "	"	"
263 ♂	" "	November 28, 1914	"
264 ♀	Wistar, 3♂ x 4♀	November 25, 1914	"
265 ♂	" "	"	"
266 ♀	Granby, 251 ♂ x 252 ♀	November 28, 1914	"

* Purchased of Abbie E. C. Lathrop, Granby, Mass.

INBRED STOCK

Number of rat	Source and generation of inbreeding	Date of birth	Experiment begun
201 ♂	Wistar, 14th	August 8, 1914	October 14, 1914
202 ♀	"	"	"
203 ♂	"	"	"
204 ♀	"	"	"
5 ♂	"	September —, 1914	November 5, 1914
6 ♀	"	"	"
7 ♂	"	"	"
8 ♀	"	"	"
211 ♂*	Wistar, 15th	December 18, 1914	April 11, 1915
212 ♀*	"	"	"
213 ♂*	"	"	"
214 ♀*	"	"	"
215 ♂*	"	"	"
216 ♀*	"	"	"

* Offspring of 201 ♂ x 202 ♀.

METHOD OF INQUIRY

Observations were made for above strains of rat as nearly as possible at the same age, and for the sake of comparability, on the same day. Certain of the observations were made under the natural cage conditions; others, under definite experimental conditions and for very specific purposes. These two kinds of data, contrasted as the naturalistic and the experimental, tend to supplement one another.

The naturalistic type of observation includes (1) observation

TABLE 1—*Continued*

Date	Number of rat	Position forward	Male and female together
"	265 ♂	$\frac{57}{60} = .95$	$\frac{27}{60} = .45$
Nov. 5 on	1 "	$\frac{65}{90} = .72$	$\frac{9}{90} = .10$
"	3 "	$\frac{43}{80} = .54$	$\frac{23}{80} = .29$
Average		$\frac{437}{700} = .62$	$\frac{98}{700} = .14$
Oct. 15 on	252 ♀	$\frac{82}{110} = .75$	$\frac{9}{110} = .08$
"	254 "	$\frac{71}{100} = .71$	$\frac{7}{100} = .07$
Mar. 2 on	262 "	$\frac{47}{60} = .78$	$\frac{12}{60} = .20$
"	264 "	$\frac{10}{60} = .17$	$\frac{18}{60} = .30$
"	266 "	$\frac{57}{60} = .95$	$\frac{27}{60} = .45$
Nov. 5 on	2 "	$\frac{76}{90} = .84$	$\frac{9}{90} = .10$
"	4 "	$\frac{65}{90} = .72$	$\frac{23}{80} = .29$
Average		$\frac{490}{700} = .70$	$\frac{98}{700} = .14$

POSITION OF RATS IN CAGE

INBRED RATS

Date	Number of rat	Position forward	Male and female together
Oct. 15 on	201 ♂	$\frac{40}{110} = .36$	$\frac{43}{110} = .39$
"	203 "	$\frac{40}{120} = .33$	$\frac{24}{50} = .48$
Mar. 2 on	211 "	$\frac{26}{30} = .87$	$\frac{11}{30} = .37$

the animals in the cage and their relation to one another may be stated thus:

(1) Outbred individuals more frequently come forward in the cage than inbred.

(2) Females more frequently come forward than males.

(3) Individual differences are greater than are the differences between the two strains or the two sexes.

(4) Inbred males and females are found together 3 times as frequently as are outbred males and females.

ACTIVITY

The degree of activity of the several rats, as indicated by their walking, running, climbing, washing, sniffing, seeking for food, lifting the lid of the cage, and so on, was observed and roughly graded by means of the numerals 0 to 5, 0 indicating minimum activity and 5 maximum activity.

As in the previous case, tabular presentation is possible, and in table 2 appear the comparable data for the two strains. In this table, the numerator of the fraction is the sum of the various grades given an animal in the total number of observations, which appears as the denominator of the fraction. The average grade in decimals in each case follows the fraction.

TABLE 2

AMOUNT OF ACTIVITY OF RATS

Outbred			Inbred		
Date	No. of rat	Activity	Date	No. of rat	Activity
Oct. 15 on	251 ♂	$\frac{286}{110} = 2.6$	Oct. 15 on	201 ♂	$\frac{163}{110} = 1.5$
"	253 "	$\frac{221}{100} = 2.2$	"	203 "	$\frac{103}{120} = .9$
Mar. 2 on	261 "	$\frac{145}{60} = 2.4$	Mar. 2 on	211 "	$\frac{66}{30} = 2.2$
"	263 "	$\frac{103}{60} = 1.7$	"	213 "	$\frac{59}{30} = 2.0$
"	265 "	$\frac{94}{60} = 1.6$	"	215 "	$\frac{70}{30} = 2.3$

SAVAGENESS OR VICIOUSNESS AS INDICATED BY THE TENDENCY TO BITE

As a means of testing the savageness of the rats, a copper wire was thrust into the cage from the front and from above, and the floor was scraped or scratched with it. Some individuals would, at this, dash forward and bite viciously and persistently at the wire, whereas others merely noticed the disturbance and were otherwise indifferent to it. The method of grading this behavior was similar to that used in the case of activity.

The essential statistical data from these observations appear as table 3.

TABLE 3

SAVAGENESS OR VICIOUSNESS (BITING) OF RATS

Outbred			Inbred		
Date	No. of rat	Savageness (biting)	Date	No. of rat	Savageness (biting)
Oct. 15 on....	251 ♂....	$\frac{92}{110} = .84$	Oct. 15 on....	201 ♂....	$\frac{31}{110} = .28$
"	....253 "	$\frac{4}{100} = .04$	"	....203 "	$\frac{109}{120} = .91$
Mar. 2 on....	261 "	$\frac{2}{60} = .03$	Mar. 2 on....	211 "	$\frac{79}{30} = 2.63$
"	263 "	$\frac{1}{60} = .02$	"	....213 "	$\frac{74}{30} = 2.47$
"	265 "	$\frac{48}{60} = .80$	"	....215 "	$\frac{26}{30} = .87$
Nov. 5 on....	1 "	$\frac{10}{90} = .11$	Nov. 5 on....	5 "	$\frac{6}{80} = .08$
"	.... 3 "	$\frac{34}{80} = .43$	"	.... 7 "	$\frac{35}{90} = .39$
Average..............		$\frac{226}{700} = .32$	Average..............		$\frac{763}{700} = 1.09$
Maximum..............		.84	Maximum..............		2.63
Minimum..............		.02	Minimum..............		.08
Oct. 15 on....	252 ♀	$\frac{6}{110} = .05$	Oct. 15 on....	202 ♀	$\frac{86}{110} = .78$
"	....254 "	$\frac{52}{100} = .52$	"	....204 "	$\frac{161}{50} = 3.22$

the time did not permit of the development of a suitable form of apparatus, this plan was abandoned in favor of a less expensive and cruder preliminary mode of observation.

The apparatus finally devised and used consisted of a small blackened rectangular box which rested at one end on two pointed metallic posts and was suspended at the other end by a delicate spring. This box was connected with a kymograph by means of a marking lever so that any vertical movement of the box was recorded on the kymograph surface. The rat was placed in the box and so confined by means of movable partitions that it was forced to hold its orientation with head pointed forward toward the writing lever. The front end of the box consisted of a wire screen. On the kymograph three records were written: (1) A time line, indicating fifths of a second; (2) a stimulus line; (3) a response line.

The only mode of stimulation here reported is the auditory, and for this purpose an electric bell was used.

The reaction box and stimulus apparatus were enclosed in a large pasteboard box in order that the animal should be somewhat protected from disturbing conditions.

QUICKNESS OF RESPONSE TO AUDITORY STIMULI

A rat having been placed in the apparatus and allowed to become accustomed to its position, the various parts of the mechanism were carefully adjusted, and when everything was in readiness an auditory stimulus was given for .5 to .6 of a second. After eight seconds, the stimulus was repeated. Then the experimenter waited for an interval of another eight seconds before again presenting a pair of auditory stimuli.

The quickness of response was indicated by the distance between the point of stimulation on the stimulus line and the point of initial response on the reaction line. The measurement is extremely crude and inaccurate, but so far as may be judged, not more so for the one strain or the one sex than for the other. Where, because of long delayed or indefinite response, it was difficult to decide on the initial point, the reaction was ignored.

The data of table 4 include the mean or average reaction time for each individual in the first trial, that is, after initial stimulus, and in the second trial, that is, with repetition of the stimulus, the maximal and minimal reaction times, the total

INBRED

Number of rat	First trial					Second trial				
	Mean	Max.	Min.	No resp.	No. of trials	Mean	Max.	Min.	No resp.	No. of trials
201 ♂	.42"*	.7"*	.08"*	0	10	.33"*	.7"*	0*	1	10
211 "	.18"	.3"	0	3	16	.26"	1.2"	0	3	16
213 "	.19"	.3"	0	1	16	.14"	.3"	0	9	16
215 "	.17"	.4"	0	2	16	.13"	.2"	0	7	16
	Aver. .18"	.4"	0	Total 6	Total 58	Aver. .18"	1.2"	0	Total 20	Total 58

Average reaction time for the first and second trials........... = .18"
Total number of failures to respond in the first and second trials = 26

Number of rat	First trial					Second trial				
	Mean	Max.	Min.	No resp.	No. of trials	Mean	Max.	Min.	No resp.	No. of trials
202 ♀	.26"*	2.0"*	.01"*	1	10	.30"*	.6"*	0	3	10
212 "	.23"	.85"	0	3	16	.22"	.65"	0	4	16
214 "	.29"	.5"	0	8	16	.08"	.08"	0	13	16
216 "	.28"	.8"	0	6	16	.23"	.8"	0	5	16
	Aver. .27"	.85"	0	Total 18	Total 58	Aver. .18"	.8"	0	Total 25	Total 58

Average reaction time for the first and second trials........... = .23"
Total number of failures to respond in the first and second trials = 43

* Excluded from the averages as the accuracy of measurements was uncertain.

(1) Inbred rats respond more quickly to the auditory stimulation than do outbred. (a) Inbred males respond most quickly of all (.18 seconds). (b) Inbred females rank next in quickness of response (.23 seconds). (c) Outbred females rank third (.39 seconds). (d) Outbred males are slowest of all (.40 seconds).

(2) The number of failures to respond obviously to the auditory stimulation is both smallest and greatest for the males. (a) The inbred males failed to respond 26 times in 116 trials. (b) The outbred females failed to respond 39 times in 116 trials.

Number of rat	First trial					Second trial				
	Mean	Max.	Min.	No resp.	No. of trials	Mean	Max.	Min.	No resp.	No. of trials
252 ♀	65.6	280	0	3	10	39.7	168	0	1	10
262 "	24.0	120	0	5	17	11.5	88	0	9	17
264 "	7.9	48	0	5	16	6.0	20	0	4	16
266 "	15.4	48	0	5	13	1.4	14	0	9	13
	Aver. 28.2	280	0	Total 18	Total 56	Aver. 14.7	168	0	Total 23	Total 56

Average amount of response for the first and second trials..... = 21.5 c.c.
Total number of failures to respond in the first and second trials = 41

INBRED

Number of rat	First trial					Second trial				
	Mean	Max.	Min.	No resp.	No. of trials	Mean	Max.	Min.	No resp.	No. of trials
201 ♂	18.1	36	2	0	16	11.3	72	0	1	16
211 "	13.9	50	0	3	16	2.9	12	0	5	16
213 "	24.1	120	0	4	16	7.0	40	0	10	16
215 "	27.9	180	0	3	16	8.2	80	0	8	16
	Aver. 21.0	180	0	Total 10	Total 64	Aver. 7.4	80	0	Total 24	Total 64

Average amount of response for the first and second trials..... = 14.2 c.c.
Total number of failures to respond in the first and second trials = 34

TABLE 6

BEHAVIOR DURING EXPERIMENT

OUTBRED

Number of rat	With intermittent stimulation				With continuous stimulation			
	Mean	Max.	Min.	No. of trials	Mean	Max.	Min.	No. of trials
251 ♂	17.4*	19.0	17.0	10	18.1	20.0	17.1	3
261 "	17.2	17.5	17.0	18	17.2	18.5	17.0	18
263 "	17.3	19.0	17.0	17	17.2	17.8	17.0	17
265 "	17.6	21.8	17.0	15	17.3	18.0	17.0	15
	Average 17.4	21.8	17.0	Total 60	Average 17.5	20.0	17.0	Total 53

Average of both responses to intermittent and continuous stimulations = 17.5

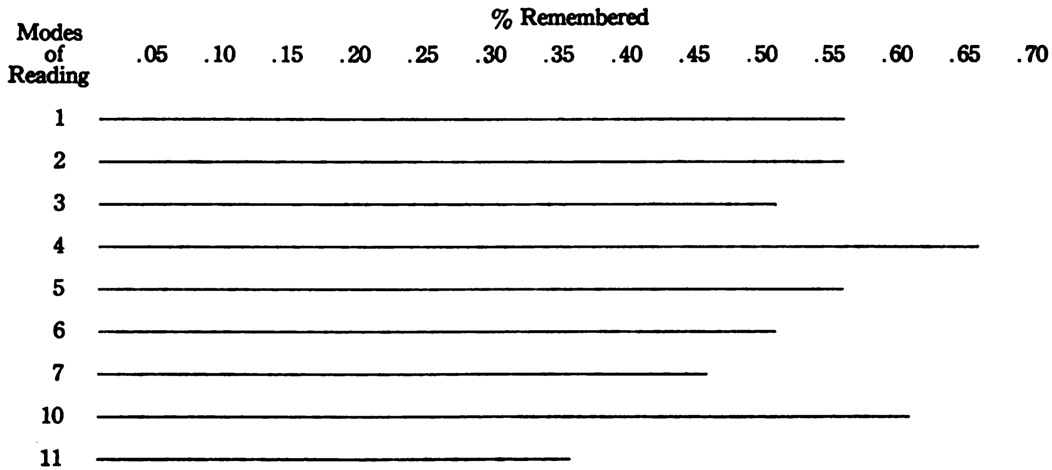
Number of rat	With intermittent stimulation				With continuous stimulation			
	Mean	Max.	Min.	No. of trials	Mean	Max.	Min.	No. of trials
252 ♀	22.5	38.2	17.0	10	23.3	26.2	20.5	2
262 "	17.4	18.0	17.0	18	19.5	38.5	17.0	18
264 "	18.4	22.7	17.0	17	18.4	21.0	17.0	17
266 "	17.3	18.5	17.0	14	18.1	21.0	17.0	14
	Average 18.9	38.2	17.0	Total 59	Average 19.8	38.5	17.0	Total 51

Average of both responses to intermittent and continuous stimulations = 19.4

* Expressed in centimeters.

TABLE III—TABLES I AND II COMBINED

RELATIVE IMPRESSIVENESS AS SHOWN BY TWO SERIES COMBINED—FOUR READINGS FOR EACH MODE



Discussion of Results

The foregoing results will be discussed under the following two heads:

I. The Function of Changes in the Use of the Four Attributes of Sound in Oral Reading.

II. Individual Differences in the Responses of Auditors.

Series I and Series II are alike in technique and material; Series II was added in the conviction that this type of experiment needs a large number of cases for validity of results.

I. Functions of Variation in the Use of the Four Attributes of Sound in Oral Reading

A. *Function of Median Range of Variation*

a) Mode 1, i. e., Median Range of All Four Attributes, holds the median position in relative impressiveness of the modes of reading employed, in Series II and in the total summary for both series. The same is practically the case for the 11 modes of Series I.

B. *Function of Extreme Range of Variation*

a) Extreme variation of Quality, in the only instance in which it was used, Series I, seems to rank high in impressiveness. Extreme Range of Time (Mode 5) gives results that are inconclusive; while Extreme Range of Intensity (Mode 7) is low enough to suggest that for this kind of situation it is not impressive.

b) When extremes of all attributes are used, the impressiveness ranks high; Mode 10 is the highest of all in the final summary, as well as in all other tables and graphs except one. (Graph 2.)

c) The one notable exception to the superiority of Extreme Change over No Change is in the matter of Time, Modes 4 and 5. Mode 4, No Change of Time, tends to rank higher than Mode 5, Extreme change of Time. But conclusions based on this condition must not overlook certain characteristics of the audience and of the stimulus.

C. *Function of No Change in Attributes*

a) The three lowest modes are all of the type that eliminates change of one or all attributes of sound. The least impressive of all modes is that which eliminates all change, Mode 11.

D. *Functions of Each Attribute Separately Considered*

a) *Pitch*. Mode 2 (Extreme change of Pitch) shows a higher rank than Mode 3 (No change of Pitch) in Series I

d) Subject 1 is an exceptional case. His reports indicate that he is set to apprehend under all circumstances and that he retains well what he apprehends. The latest trials of the experiment indicate that he had become adapted to all modes of reading, no matter how foreign to his previous experience.

e) All women subjects—numbers 22, 24, 26, 27—rank well up in the total average.

f) The chief divergences in ability to report are shown by the male subjects, ranging as they do from .15 to .82.

Conclusions

1. There is a presumption in favor of using an extreme degree of change in all four of the attributes of sound during speech, especially for the purpose of securing retentiveness over an extended time.
2. The four attributes of sound differ in their effect upon the responses to oral reading.
3. Individuals differ widely in their responses to a given combination of change activities.
4. Auditors tend to become adjusted to unconventional modes of reading.

Subsidiary conclusions are:

a) In intellectual impression (such as this type is) regulation of Pitch changes is significant: a wide range is preferable to no change.

b) An even rate is superior to a rate excessively broken.

c) Interference with normal degree of change in Intensity reduces apprehension, retentiveness, or both.

d) Elimination of changes in Quality (Timbre) is greatly inferior to a wide degree of change.

General Conclusion as to Type of Experimentation. As to whether this method offers promise of valid results for further use, the following evidence is offered. (1) A judgment practically unanimous among students of oral expression is that changes in Pitch are always significant, that absence of all change is detrimental to intellectual impressiveness, and that the most effective reading is with an abundant, even extreme, range of change in all four elements of sound. This study brings results precisely in accord with these judgments. (2) Among authorities in elecution there seems to be no unified opinion as to the effect of changes in Time; and this study leaves that issue with results that are inconclusive, calling for refinement of method or more data. (3) Interference with changes in Intensity so as to affect accentuation, could be

assumed empirically to be detrimental to impressiveness; and the experiment reveals just such a result. (4) Changes in quality are commonly assumed to represent changes in emotion, mood, total attitude, and hence highly impressive. This study strongly confirms this judgment.

Hence it is a justifiable conclusion that a method which brings results that conform with generally accepted judgments on matters of widespread experience, will prove useful in deciding ultimately such issues in this field on which opinion is not as yet in agreement.

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METHODS OF EXHIBITING REACTIVE TENDENCIES CHARACTERISTIC OF ONTOGENETIC AND PHYLOGENETIC STAGES

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Methods which have contributed importantly to our knowledge of the ontogeny and phylogeny of reactive tendencies, and more especially to those types of adaptive behavior which we call ideational, are few and unsatisfactory. Only recently have experimental devices and procedures been suggested which are alike suited to reveal the reactive tendencies of ontogenesis and phylogenesis and to stimulate interest in genetic description of behavior.

Following a brief historical sketch, I shall describe an apparatus by means of which three of the most recent and promising of our behavioristic methods may be used.

From the birth of interest in the problems of psychogenesis, about the middle of the last century, until the end of the century, no scientific means of approaching the problems of ideational behavior[1] were developed. Romanes, Brehm, Morgan, and their psychological contemporaries who happened to be interested in evolutionary or genetic problems worked either from anecdotal materials or from observations gathered by the use of crude and unstandardized methods which may fairly be characterized as wholly unsuited to scientific inquiry. We regard their contributions to genetic psychology as suggestive of possibilities of research or as defining problems rather than as important additions to our knowledge of fact.

With the appearance of Thorndike's mental initiative, the situation radically changed, for the puzzle-box or problem method came into existence and began to be used systematically as a

[1] I shall designate as ideational behavior those forms of adaptive response which in objective characteristics are identical with, or strikingly resemble, what we appropriately and with common consent call ideational behavior in man.

gradually perfected his method, the while applying it to various ontogenetic stages in man, cat, dog, and monkey, to defective and deranged human adults, and to many and diverse types of animal.

The Hamilton method, which, in the opinion of the writer, is equal in importance to any method of studying behavior yet proposed, has been almost wholly neglected by comparative psychologists and its results are very imperfectly known.

While Cole and Hamilton were busy with their new methods, Carr and Hunter[1] were perfecting, in the study of the white rat, what has appropriately been termed the method of delayed reaction. It is a simple and ingenious way of testing for ideation. Like Hamilton's, Hunter's contribution to our science is important methodologically as well as for its factual materials. But whereas Hamilton's method of quadruple choices is suited to reveal reactive tendencies and to exhibit their genetic relations, Hunter's serves primarily as a test of the ability of an organism to respond to a situation from which the significant feature (stimulus) has vanished.

For purposes apparently foreign to the interests of both Hamilton and Hunter, the writer a few years ago devised yet another method of studying ideational and other reactive tendencies. It has been called the method of multiple choices. It was planned as a means of gathering strictly comparable data of reaction from diverse types of organism, stages of development, and conditions of normality or abnormality. It was the writer's hope and conviction that most varied scientific materials should be assembled systematically in the interest of genetic description. The method is therefore appropriate to human psychology and to infrahuman, to child psychology and to psychopathology.

To sum up:—for reasons which are obvious to every careful student of behavioristic method and result, Hamilton's method of quadruple choices is a preëminently valuable means of displaying reactive tendencies; Hunter's is an uniquely serviceable test of ability to respond appropriately to controllable absent stimuli; and the writer's is a promising mode of evoking varied types of response and of reactive tendency for purposes of classification and more detailed analysis.

[1] The method is hereafter referred to as Hunter's because he alone has published concerning it.

the apparatus) connected by means of cords or wires with the various entrance and exit doors of the apparatus, and so arranged as to enable the experimenter to unlock and open or to close and lock any given door by a simple movement of a key or lever; (7) a protected incandescent lamp in each of the boxes, with the necessary switch and timing mechanisms for its satisfactory use in connection with the Hunter method of delayed reaction (lamps need not be installed in the twelve boxes, but only in those which are to be used for the delayed reaction method).

This apparatus may be built in three sizes: small, medium, and large.

The small apparatus is suitable for experiments with such organisms as the toad, frog, lizard, tortoise, mouse, rat, sparrow, canary, and other like-sized amphibians, reptiles, birds, or mammals. The medium-sized apparatus is suited for experiments with the tortoise (large), snake, dove, crow, domestic fowl, cat, small dog, raccoon, rabbit, squirrel, marmoset, and other medium-sized reptiles, birds, or mammals. The large apparatus may be used for various types of large-sized lower vertebrates, and for such mammals as the cat (large), dog, pig, goat, sheep, bear, monkey, ape, and man.

The several figures indicate the general plan of the apparatus and certain of the most important points of construction.

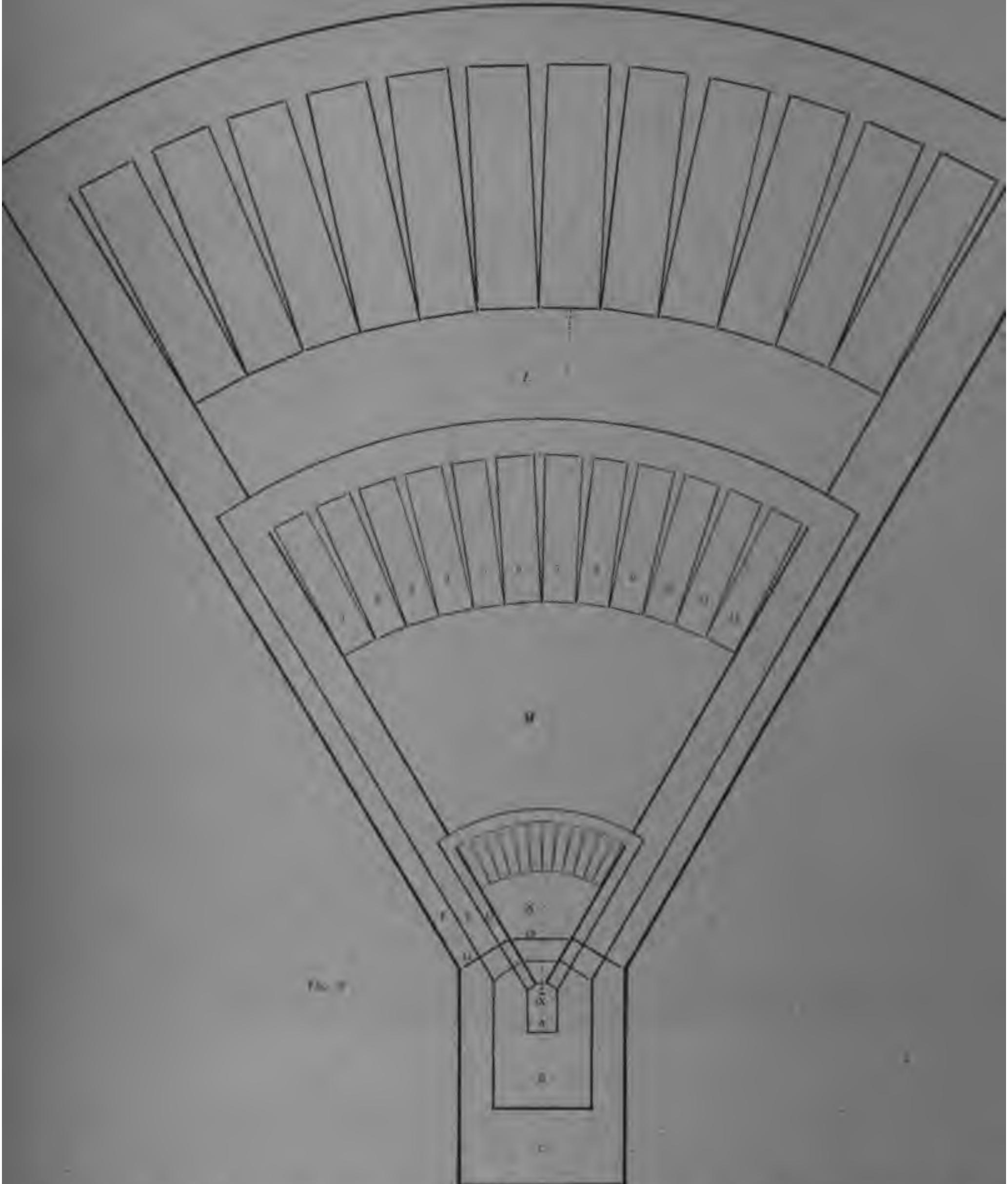
Each reaction box, according to figures 1 and 3, and also according to the measurements of table 1, occupies five degrees of arc. The width of the box is therefore determined by its distance from the center X (figures 1 and 3). By making the boxes intercept six degrees instead of five, the advantage can be gained of shorter distances between release door and entrance door, but there results the serious disadvantage that the apparatus is so spread out as to demand a considerable eye movement for inspection of the twelve reaction boxes. There is the further disadvantage, in the wider angle, that the large apparatus requires for its installation a floor area of nearly thirty-six by thirty-six feet. For these and other reasons, it has seemed desirable to make use of the five degree angle in the designing of this convertible apparatus.

The alleys are, in each size of apparatus and throughout their lengths, the same width inside as the reaction boxes are outside.

The plan of the medium sized apparatus appears as figure 1, and in figure 2 there is shown an enlargement of one of the reaction boxes, with the arrangement of sliding entrance and exit doors and the concealed reward mechanism. Figure 3 represents the three sizes of apparatus in their relations. These must, of course, be built separately and be independent of one another.

FIGURE 2.—Ground plan of reaction box. En, entrance door; Ex, exit door; s, s, wooden guides for sliding door; B, wooden block for food cup; R, food cup.

The small apparatus should be made of quarter inch white wood (poplar), red wood, or pine, according to locality, and covered with netting made of No. 20 wire, three meshes to the inch. The medium sized apparatus should be made of half inch stock, and the wire netting used as a covering, or for other necessary purposes in connection with it, should be No. 17 wire, two meshes to the inch. The large apparatus should be made of seven-eighths inch stock, and the accompanying wire netting should be made of No. 12 wire, one mesh to the inch.



[FI]GURE 3.—General plan for three sizes of reactive tendency apparatus. S, small apparatus; M, medi[um appa]ratus; L, large apparatus. X, center of circles on arcs of which reaction boxes and outer alley [are] placed; A, release box for small apparatus; B, release box for medium apparatus; C, release box [for large] apparatus; D, E, F, alleys for small, medium, and large apparatus, respectively; 13, release door (t[he] doors for the three sizes of apparatus are shown); 14, door between release box C and alley F.

All stock should be planed on both sides, and the apparatus should be given two or three coats of dark gray paint, if it is to be exposed to the weather. If, instead, it is to be used indoors, it should be painted white or gray, according to the degree of illumination of the experiment room.

The walls of the reaction chamber should be made of wire netting of the weight indicated above. The outer walls of the alleys may be made of wood or wire netting. The release box should be built of wood except for the wire netting cover and door. The entrance and exit doors should be made of wood.[1]

In table 1 are presented the chief dimensions for the three sizes of apparatus under consideration.

TABLE 1

Principal Dimensions in Centimeters or Inches of Convertible Reactive Tendency Apparatus

Measurements	Dimensions for Small	Dimensions for Medium	Dimensions for Large
Of reaction boxes			
Width outside	10 cm.	30 cm.	60 cm.
Width inside (minimum)	7.5 cm.	25 cm.	51 cm.
Length outside	30 cm.	60 cm.	140 cm.
Length inside	29– cm.	58– cm.	135– cm.
Depth outside	20 cm.	40 cm.	200 cm.
Depth inside	19+ cm.	38+ cm.	198– cm.
Of entrance and exit doors			
Width	8.4 cm.	27 cm.	54 cm.
Length	20 cm.	40 cm.	200 cm.
Of release box			
Width	33+ cm.	99+ cm.	198+ cm.
Length	30 cm.	60 cm.	140 cm.
Depth	20 cm.	40 cm.	200 cm.
Of release box doors			
Width	10 cm.	30 cm.	60 cm.
Length	20 cm.	40 cm.	200 cm.

[1] For details see *Behavior Monographs*, vol. 3, no. 1, p. 14.

Measurements	Dimensions for Small	Di M
Of alleys		
Width inside..........	10 cm.	30
Depth................	20 cm.	40
Distance from center X to entrance doors.....	114.5 cm.	343
Distance from release door to entrance doors.....	105.9 cm.	317
Of strips for doors to slide in		
Thickness............	1/4 in.	1/
Width................	2 cm.	3
Length...............	20 cm.	40
Block for reward mechanism		
Width................	6 cm.	1
Length...............	10 cm.	30
Depth................	2 cm.	4
Hole in block		
Diameter............	4+ cm.	6
Food cup		
Diameter at top......	4 cm.	6
Depth................	2 cm.	4
Cover for food cup		
Width................	7 cm.	20
Length...............	8 (2+6)	1-
Space necessary for apparatus in use		
Width................	10 ft.	20
Length...............	12 ft.	20

Certain suggestions concerning details
practical importance. It is desirable, for t
to supply each box with a floor. This floo
than the sides of the box so that the en
may drop past it, thus discouraging att
raise the doors. Or, if the floor is cut full
across the box just inside of the exit doo
purpose while giving support to the floor
Each box should have a wire netting co

All doors should slide vertically, upward, in wooden ways. These are conveniently made by nailing strips of wood to the side walls of the box. The strips serve the additional purpose of supporting the side walls. The outside strip may either be nailed to the end of the side wall or along the side. If nailed to the end, it serves as the outside strip for adjacent doors and thus reduces the amount of labor. In figure 2, the outside strip for the entrance door is shown as nailed to the end of the side wall. The writer prefers this method of construction.

The reward receptacle, or mechanism, must be so constructed as to be concealed when the exit doors are down and fully exposed when they are raised. It may be simply and conveniently constructed by nailing outside the rear end of each box a block of wood, of the dimensions suggested in the table, in the center of which there is a hole large enough to receive a metal food cup. Aluminum is preferable as material for the food cup, and desirable dimensions for the various sizes of apparatus are suggested in table 1. In the proper position on the outside of the exit door, there should be screwed a metal plate, bent at right angles in such wise as to cover completely and tightly the food cup when the exit door is down. This is shown in figure 4.

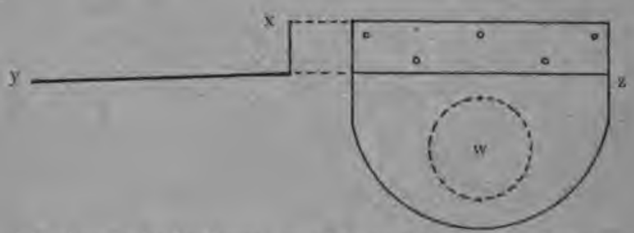


FIGURE 4.—Metal cover for food cup. w, position of food cup under cover; z, point at which cover is bent nearly at right angles; x, portion of cover which is attached to exit door by means of wood screws, holes for which are indicated; y, portion of cover which hides food cup.

The dimensions for this cover or cap for the food cup, also, are indicated in table 1. For the small apparatus, heavy tin is a satisfactory material for this cover; for the medium apparatus, light galvanized iron suffices; and for the large apparatus, it is necessary to use galvanized iron which is so thick that the large apes cannot readily bend the cover out of shape. The thickness should be about 1/16 inch.

For most animals there is no necessity of locking the doors of the apparatus, but when it is to be used with monkeys or anthropoid apes, it is absolutely necessary that the experimenter be able to securely lock any one or all of the sliding doors. It is therefore essential to equip the large sized apparatus with locks to be operated in connection with the mechanisms which raise and lower the doors. Each door should lock automatically when lowered and unlock when the raising mechanism is operated.

Just behind and a trifle above the release box, an observer's stand or record table should be constructed, separated by a screen from the apparatus so that the animal shall not be able to see the observer. On this table there should be placed a keyboard, or lever device, by means of which any one of the twenty-six working doors[4] of the apparatus may be raised or lowered quickly and quietly.

For the small apparatus the various doors may be controlled readily by means of a light cord, which runs from a screw eye in the top of each door, through appropriately placed pulleys, to a hinged lever key which the observer operates. This key should be so arranged that when it stands in approximately vertical position the entrance door is closed. When it is placed in the horizontal position, the entrance door is open. A cord from the exit door, carried similarly by pulleys, should be so placed that it may be attached readily by means of hook and ring, or ball and slot, to this key, so that if, when a given entrance door is lowered, the experimenter desires to raise, simultaneously, the exit door of the same box, the pushing of the key to the vertical position will effect the appropriate movement of each door, that is, will simultaneously lower the given entrance door and raise the given exit door. The distance to which the entrance door is raised may be altered by changing the point of attachment of the cord to the key. This simple hinged key and cord device renders necessary the use of only fourteen keys for the operating of twenty-six doors, but the scheme is feasible only so long as the doors in question are light enough to be readily moved by means of a fairly small lever key. The accompanying diagram, figure 5, indicates the relations of parts, as described above.

[4] If both return alleys are used there are twenty-seven doors instead of twenty-six to operate.

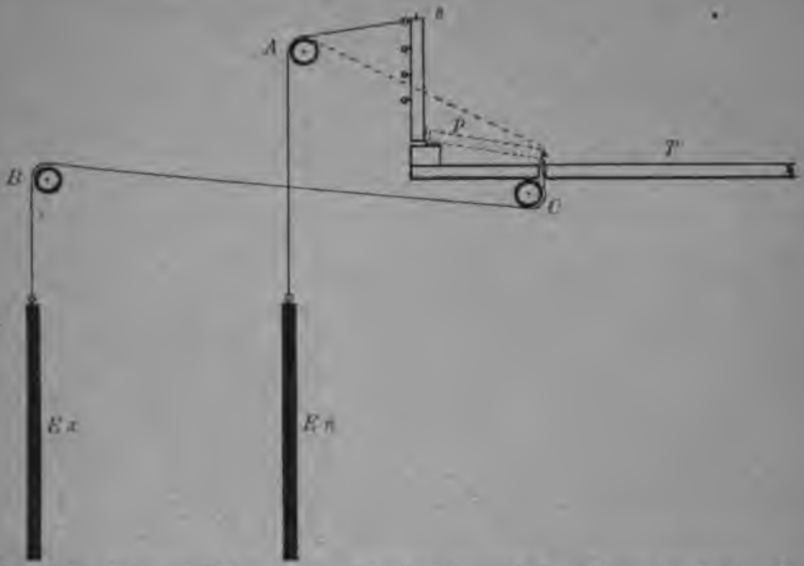


FIGURE 5.—Diagram of lever-key mechanism for raising and lowering doors. En, entrance door; Ex, exit door; T, observer's table; s, hinged lever key in vertical position; p, same, in horizontal position; A, pulley for cord between entrance door and lever key; B, pulley for cord between exit door and lever key; C, second pulley for cord from exit door.

For the medium sized apparatus also, the lever key mechanism is feasible, but it requires considerably more space and much greater effort on the part of the experimenter. A substitute for it is the weighted cord mechanism.[5] A cord with appropriate carrying pulleys is provided for each door, and to the end of the cord, which drops in front of the experimenter's table and within easy reach, is attached an iron or lead weight which is just sufficient to hold the door in position after it has been raised by the experimenter. If the weight is too heavy, the door will tend to rise at inappropriate times; if too light, it will not stay in position after being raised. This device has the defect of varying in reliability with humidity and temperature, since the door will slide more or less easily in accordance with these varying conditions. The lever mechanism is preferable,

[5] Described in previous papers on the multiple-choice method. A study of the behavior of the pig *Sus Scrofa* by the multiple-choice method, *Journal of Animal Behavior*, 1915, **5**, p. 188. The mental life of monkeys and apes: a study of ideational behavior, *Behavior Monographs*, 1916, **3**, p. 14.

since it can be relied upon to place and hold the doors in a constant position.

For the large apparatus, it is extremely desirable to devise some type of lever mechanism which shall be easily manipulated, reliable, and inexpensive. All of the mechanisms thus far proposed are either too cumbersome or too expensive to be feasible, but it is hoped that shortly a method may be discovered by which the experimenter may conveniently and accurately control the various doors by means of levers, the maximum excursion of which shall not exceed eighteen inches. Since the various doors must be raised a maximum of seventy-two inches, it will probably be necessary to introduce one or more forms of multiplying device. Already an automatic locking device, to be operated in connection with the proposed system of levers, has been designed.

In the absence of a satisfactory scheme for the use of levers, weighted cords and locks, which are operated independently, may be employed. But this system of control mechanism, as has been stated above, is both unreliable and troublesome to operate because of the numerousness of the parts. There must be a separate weighted cord for each of the twenty-six doors and a separate lock mechanism for each of the twelve boxes, entrance and exit door in each case being controlled by the same lock.

USE OF APPARATUS

The use of the convertible reactive tendency apparatus in connection with each of the three methods in question will now be described. For all of the methods alike, rewards and punishments may be used as inducements to effort. As rewards, food presented in the food cups, or for children small presents similarly presented, serve well. In certain exceptional instances, it may prove desirable to present the reward for a successful choice, not in the food cup of the correct box, but instead at the entrance to the release box. As punishment, it has proved feasible to use confinement in incorrect boxes. It seems probable that for certain organisms the electric shock may prove useful.

Hamilton Method

For use with the Hamilton method of quadruple choices, the following procedure is suggested. This method involves the use of only four reaction mechanisms. Boxes 5, 6, 7 and 8 may

therefore be used, the fact that they are to be reacted to being indicated by their openness, the entrance doors being raised in case of each trial. Since the entrance doors of all other boxes should remain closed and locked, there would be no persistent tendency on the part of most organisms to attempt to enter other than the four boxes referred to. For some purposes, it may prove even more satisfactory to use boxes 2, 5, 8 and 11.

Incorrect choices would not be rewarded, and as seemed desirable the subject could be punished for such choices by being confined in the boxes for a stated period. A correct choice, no matter what the particular form of the problem, would naturally be rewarded by the presentation of food in the food cup.

Various problems, in addition to that originally suggested by Hamilton, may be presented by this method. The following will suggest the range of possibilities: (1) An insoluble problem, such as Hamilton used, the several boxes serving as correct boxes in irregular order, but the same one never twice in succession and each the same number of times in every hundred trials (this problem is practically insoluble by even the most intelligent organism); (2) the systematic use, as correct box, of each in turn from the left end to the right end, that is, 5, 6, 7, 8, or in case of the other group of boxes, 2, 5, 8, 11, this succession being repeated indefinitely; (3) box at left end, box at right end, box next to left end, box next to right end, the same being repeated indefinitely. From these suggestions, it is evident that various degrees of complexity of order and relationship might be utilized to elicit reactive tendencies and to display problem solving ability of different sorts.

The apparatus demands no special modification or adaptation for use in connection with the Hamilton method. Further details are unnecessary in view of the fact that Hamilton has already published a fairly complete description of method and apparatus,[6] and has in press a still more elaborate account of procedure and results.[7]

Hunter Method

For the method of delayed reaction the apparatus demands certain special appliances which, however, do not have to be removed when either the Hamilton or the Yerkes method is

[6] Hamilton, G. V. A study of trial and error reactions in mammals. *Journal of Animal Behavior*, 1911, **1**, pp. 33-66.

[7] *Behavior Monographs*, 1917, **3**, no. 13.

in use. The special equipment consists of a concealed incandescent electric lamp for the illumination of each box and an electric signal and timing mechanism for the operation of the lamps and the door between the release box and the reaction chamber.

The method of delayed reaction may be used with various groups of doors, according to the grade of difficultness of response desired. Thus, as the simplest situation, boxes 6 and 8 may be used. In this case, the entrance doors of both boxes should be raised in preparation for a trial. The doors of the other boxes should remain closed. In accordance with a prearranged plan, either the one or the other box would be indicated, by momentary illumination, as the box to be chosen.

For the second grade of difficultness, boxes 5, 6, 7 and 8 might be used, each of them having the necessary equipment and connections for use as the correct box; for grade three, boxes 2, 5, 8 and 11; for grade four, boxes 1, 3, 5, 7, 9 and 11; and for grade five, all of the twelve boxes might be subject to use, that is, the entrance door of every box should be open and the subject should be required to choose that one of the twelve which has previously been illuminated.

The satisfactory use of this method necessitates not only the presence of a lamp, but the installation of a mechanism which shall control several important factors in the situation. The experimenter, by pressing a simple key, should close a circuit which at once illuminates a certain box (the particular box to be determined by the setting of a switch), and at the same time starts a timing mechanism. This mechanism should, after an interval, with a range of 1 to 10 seconds, open the lighting circuit, thus cutting off the illumination of the correct box; and after an interval of 0 to 60 seconds it should cause the door of the release box to open so that the animal may enter the reaction chamber. For intervals longer than 60 seconds, it seems best to have the experimenter determine the delay by means of a stop watch and operate the door of the release box by hand.

There is no obvious reason why this twelve mechanism reactive tendency apparatus should not be used in wholly satisfactory fashion for the study of delayed reactions. The additional electrical equipment should in no wise interfere with the other uses of the apparatus and that portion of it which controls the release box door might be made to serve the experimenter in connection with all of the methods.

Yerkes Method

For use by the method of multiple choices, the apparatus demands neither modification nor special adaptation. The chief features of the method have already been described several times, and it is needless here to do more than formulate a set of problems with wider range of difficultness than those heretofore used in reported experiments on lower animals. Those proposed problems, ten in number, are presented in brief form below, with a series of ten settings for each. Thus, in case of problem 1, for which the correct mechanism is always box number 5, that is the fifth from the left end of the apparatus, the first setting involves the use of boxes 1 to 6, the second setting, of boxes 3 to 12, and so on. It is understood that, if possible, this series of ten settings (ten trials) shall be presented to a subject once a day until the problem has been solved. If for any reason the series of ten trials cannot be completed on a given day, it should be resumed from the point of interruption on the following day. If more than one series per day can be given, either the ten trials may be divided into two groups of five each or the total series may be repeated.

In each of the series of ten settings, a total of sixty boxes is presented. The average number of boxes open in each trial is, therefore, six. Of these sixty boxes, ten are definable as correct boxes. The probability of correct first choice prior to experience is for any series of ten trials, one to five. In order that this ratio of probable right to wrong first choices shall not be disturbed, it is desirable that the experimenter make use of the proposed settings.

Proposed Problems and Settings for Multiple-Choice Method

Problem 1. Same box (box 5).
1-6 (5); 3-12 (5); 4-6 (5); 5-9 (5); 2-10 (5);
4-5 (5); 4-10 (5); 3-6 (5); 1-8 (5); 5-10 (5).

Problem 2. First at left end.
6-12 (6); 11-12 (11); 3-11 (3); 1-5 (1); 4-11 (4);
10-12 (10); 5-9 (5); 2-12 (2); 8-11 (8); 7-12 (7).

Problem 3. Middle.
1-7 (4); 10-12 (11); 6-10 (8); 1-11 (6); 1-3 (2);
4-10 (7); 1-9 (5); 9-11 (10); 1-5 (3); 6-12 (9).

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THE RELATION OF POINT-SCALE MEASUREMENTS OF INTELLIGENCE TO EDUCATIONAL PERFORMANCE IN COLLEGE STUDENTS

IN the hope of adding to our profitable information concerning the relation of educational performance to intellectual ability, the writers, during the present collegiate year, applied the Yerkes-Rossy point scale for the measurement of intelligence in adolescents and adults to two groups of college students. The results of the examinations, as reported below, are likely to be of interest to educators and psychologists, first, because of the positive correlation between intellectual capacity and collegiate performance; second, because of conspicuous sex differences in intellectual measurement; and third, because of the demonstration of the feasibility of group examining by the point-scale method.

The adolescent-adult point scale, the original form of which has been fully described elsewhere,[1] consists of twenty tests or simple methods of mental measurement, each of which is to be presented to every subject examined. Credit is given for each test in accordance with the nature of response; the maximum number of credits for the scale is one hundred points. The scale, as originally planned, was adapted to individual examining, but the writers, with little difficulty, adapted it to the demands of group examining. This involved

[1] Yerkes, R. M., and Rossy, C. S., "A Point Scale for the Measurement of Intelligence in Adolescent and Adult Individuals," *Boston Medical and Surgical Journal*, April 19, 1917.

the preparation of lantern slides for the materials of several of the tests and the printing of special record blanks. Since the several parts of this scale and the general method of examining have already been presented elsewhere, it will suffice, in this connection, to give a list of the various measurements, together with a statement of the mode of presenting the test material and the time allowed for presentation and for the subject's response.

Of the twenty tests, one, comparison of weights (No. 2), could not be given simultaneously to the individuals of a group, but by using several sets of weighted cubes, each cube marked with a letter, it was possible for us to get results from nearly one hundred students in an hour. The cubes were handed from subject to subject, and in each case the subject after examining the weights recorded his judgment on a card with which he had been provided.

The remaining nineteen tests, of which 1, 4, 12, 16, 18, 19 and 20 demanded the use of lantern slides, required nearly two hours' work on the part of the group. In individual examining, the scale can be presented in approximately one hour, but when the subject is required to record his own responses completely, the necessary time is increased as has been indicated above.

This group examination was given to one hundred and ten members of a class in psychology in a college for men and to one hundred and thirty members of a class in psychology in a college for women. The courses were in charge of the writers, who also conducted the point-scale as well as the academic examinations, the data of which are herein recorded. The same person conducted the psychological examination in both instances. Complete point-scale examinations were obtained for one hundred men and one hundred nineteen women. Incomplete examination

inbred males, average 18.0; outbred females, average 19.4; inbred females, average 18.2.

(5) Individual and sex differences greatly exceed strain differences.

CONCLUSIONS

The outbred and inbred strains of albino rats contrast in temperament as follows:

(1) Inbred males and females are found together in the cage about three times as frequently as are outbred.

(2) The inbred rats come forward in the cage on the approach of the experimenter much less frequently than do the outbred.

(3) Inbred rats are less active than outbred.

(4) The inbred stock exhibits savageness by biting to approximately twice the extent of the outbred.

(5) The inbred animals respond more quickly and in greater amount to momentary auditory stimulation than the outbred.

(6) The two strains differ also in restlessness or continuity of response. For the inbred restlessness is greatest in case of momentary and repeated auditory stimulation and less in case of continuous stimulation, whereas for the outbred animals, the reverse is true.

(7) The data indicate less difference between the sexes in the inbred than the outbred rats.

EFFECTS OF VARIOUS MODES OF PUBLIC READING[1]

By Charles H. Woolbert, Ph. D.

This study is an attempt to bring the numerous problems of oral expression and public reading into the laboratory. For the beginnings of such an experiment the most promising place is the laboratory of psychology inasmuch as speech is intended to reach the minds of auditors who are expected to react to the stimulation furnished by the voice of the speaker or reader. Thus the problem deals with auditory stimulations and with behavior activities of listeners. The task to be met is that of finding a method whereby the stimuli can be controlled and receptivity measured; that is, of insuring an adequate description of the effect of the voice of the speaker or reader upon the responses of the auditors.

The method adopted after many preliminaries was based upon a study of the relation between *changes* in the use of the voice and specified responses of auditors; that is, the effects of various modes of public reading, employing *different combinations of changes* in the use of the attributes of sound while reading. These different combinations offer various *modes of reading*. The attributes of sound are pitch, intensity, time, and timbre (sometimes called quality when applied to the voice). While all speech that is meaningful to auditors employs constant changes in the use of these four attributes, still there are no results obtained under laboratory conditions that show which are most significant or what relation they bear to each other. A restricted series of modes of reading was selected for use throughout the study, limited to certain *gross degrees of change* in pitch, time, intensity, and timbre. A description of these modes together with the method of evaluating results from their use, is given later.

Changes in pitch, time, intensity, and timbre can be studied as general and total or as finely modulated and of narrow degree. Again they can be studied as affecting individual words, sentences, paragraphs, or larger units of discourse.

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[1] From the Psychological Laboratory, Harvard University.

The type of change studied here applies to *large units* of discourse, in this case passages of twenty minutes in length. No attention is here paid to the modulations occurring between words, or sentences, or units of small measure. A certain "tone of voice" is selected and used throughout a twenty-minute passage.

The Attributes of Sound as Elements of Change in Reading

Pitch Changes. When employing changes in Pitch as the basis of investigation, it is necessary to hold in mind that these are of at least three different kinds: (1) the slide within the individual syllable, (2) the step from one syllable to the next, and (3) the general pitch level on which a passage or a composition is uttered. The first two are continual changes required in normal speech; the last change is only occasional. When change of pitch is referred to in this paper, change of the slide within the syllable and from one syllable to the next is intended.

Time Changes. Changes in time are chiefly in the length of the vowel parts of words, i. e., the tonal part as against noises. "Gauthiot's[2] records show that there are two main classes of vowels in respect to duration, long and short, whose average lengths bear the remarkably constant relation of 4:2."

Changes in Pause. A cause of the frequency of duration change is that pauses must be included. Pauses—periods of silence—are necessary because of (1) the need of a break between syllables and words, and (2) because of the demands of breathing. Accordingly ,in speaking here of changes in time we always include changes in pause: pauses between syllables, words, phrases, clauses, sentences, and the major divisions of composition.

Intensity Changes. Changes in tone intensity are also very common owing to the fact that accentuation is not possible, in English, without a change in intensity on the accented syllable; for accent, in this language at least, is largely a matter of a rise in pitch and an increased energy of utterance. Also emphasis, as distinguished from accent, is almost always accompanied by an increase or decrease in intensity.

Qualitative Changes. Changes in quality (timbre) are least common of all. Changes in quality are probably more influential than any other type of change in revealing the emotional

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[2] Gauthiot, De l'accent et de la quantite en literanien, *La Parole*, 1900, II., 143. (Cited by Scripture.)

state of the speaker. Emotional states involve muscular and vascular changes in the throat and pharynx, and thus the body of overtones is greatly affected in number and intensity, producing attention changes in quality.[3]

Overlapping Effect of the Four Attributes. In every manner of reading that involves a maximal change of one of the elements, there is a certain amount of overlapping. When the extremes of change in pitch are used, there is a necessary change in quality. Very high notes, lacking certain overtones, produce a different quality impression from tones possessing these overtones. A prolonged tone, especially a tone of speech necessarily involving a slide, tends to carry the slide on during the prolongation, thus involving a change in quality. A tone of strong intensity is kept loud most easily by being also high in pitch. Tones of "chest resonance" are prevailingly lower in pitch than those of "head" quality; at least the majority of people find it easier to form them in these combinations.

By isolating one attribute at a time, giving it an extreme degree of change while holding the others at a median degree, we get 4 modes of reading; by doing the same with no change of each of the attributes we get 4 more modes; by using a median change of all 4 we get another; by using an extreme degree of all four we add one more; and, finally, by employing no change at all in any of the attributes we add the last; making in all 11 modes of reading. These represent only gross situations, and so are capable of control at the hands of a trained reader. Following is a specific designation of each of these modes:

Mode 1: Median change of all four attributes.
Mode 2: Extreme change of Pitch; Median change of other three.
Mode 3: No change of Pitch; Median change of other three.
Mode 4: No change of Time; Median change of other three.
Mode 5: Ext[illegible]me change of Time; Median change of other three.
Mode [illegible] Median change of other three.
Mode 7: [illegible] M[illegible] change of other three.
Mode [illegible] Quality [illegible] three.

[3] Blanton, S. [illegible] and the Emotions.

Mode 9: Extreme change of Quality: Median change of other three.
Mode 10: Extreme change of All Four Attributes.
Mode 11: No change in any of Four Attributes.

Modes of Reading

Mode 1. Median Change of All Attributes. In this mode all changes are of a median degree, but are constant in all four of the attributes of sound. Extremes of all kinds are avoided. The general effect of this type of reading is that produced by a congregation reading a Psalm in unison. There is a lack of what might be called "brightness," "animation," "alertness." In general it would be called rather dull reading, yet far from unintelligible and not at all bizarre or unusual.

Mode 2. Extreme Change of Pitch.[4] In this mode the voice travels to extremes of height and depth continuously, in the use of the slide on the syllable and in the step from syllable to syllable. Changes are extreme not only in distance on the scale of pitch, but are frequent in occurrence, no passage of more than three or four syllables being without its abrupt and wide change. This mode is not altogether removed from meaningful and intelligible speech; for not a few sentences resemble a type of speaking frequently heard in public address and in private conversation. Yet the continual shifting of the voice up and down to extreme depths and heights produces an effect of monotony that is marked.

Mode 3. No Change of Pitch. In this mode the reader eliminates all inflection of the voice upwards or downwards on the scale whether in slide, step, or general level. In the first place, the level is approximately at the middle height of the reader's range. Then, all slides also were eliminated, and likewise the steps; so that every sound was of the same level as every other sound. This type of reading is far from meaningless and is by no means strange to listeners who are accustomed to the "intoning" customs used by certain ecclesiastical sects.

Mode 4. No Change of Time. In mode 4 every syllable was rendered with the same duration as every other syllable. Moreover, the pauses were of the same length, exception being made when breath had to be taken. A manner of speech employed by certain foreigners in learning the Eng-

[4] A characterization like this assumes, it is to be remembered, a median degree of change of the other four attributes.

lish language approximates this; also, some speakers in their anxiety to be distinct use this mode. A most noticeable effect is the staccato involved. This mode, though, is not without meaningful emphasis, inasmuch as the emphasis of slide and intensity are retained to a median degree.

Mode 5. Extreme Change of Time. Widely varying degrees of change in time were applied not only to the vowel sounds, but to the pauses. Still the prolongations in this mode were not so great as is sometimes heard on the stage and in impassioned speaking, where with such an extreme manner must go a wide range of pitch or an amplification of the quality or an increase in the intensity that would constitute an error in the use of the median range of these three demanded by the premises. This is a relatively common mode; "animated" conversation is almost always markedly broken in time.

Mode 6. No Change of Intensity. Reading on one level of intensity encounters a difficulty, in that there is need of eliminating all intensity factors from accentuation; and that is not easy. When all syllables must be made equally loud, accent disappears. Thus, choice had to be made between making unaccented sounds louder or accented sounds quieter. The latter alternative was chosen, for the reason that otherwise the whole effect would be merely that of blatant shouting. As a consequence, the reading in this mode gave the impression that the reader was very tired or uninterested in his task. Also when such accentuation by intensity change is eliminated, the listeners get the impression that the words are not being pronounced correctly and so reveal distress at first sound of the mode. Later they seem to be relieved.

Mode 7. Extreme Change of Intensity. The simple formula for describing this mode of reading is to state that the sounds that in "normal" speech are the loudest are here made very loud; those that are of a median degree of loudness ordinarily are made loud; the sounds of least intensity in every-day speech remain about the same, for they are merely audible then and cannot suffer lowering of intensity without disappearance. Listeners seemed to be benumbed by this manner.

Mode 8. No Change of Quality. To accomplish this, one system of resonance, or complexity of sound, had to be decided upon arbitrarily from the several possible in the use of a trained voice. The quality chosen is that known as "oral" in terms of elocution. It is a quality that derives its resonance

chiefly from the bones high in the head
overtones, but poor in the lower ones
impressiveness in speech. All resonan
of the pharynx" is reduced to a minim
altogether. The effect of this manner i
appear sick or weak.

Mode 9. Extreme Change of Quality
ous complexities of tone are produced by
so that there is a continual change in th
producing chambers of the head, thro
effect is what is loosely called a continu
tional" tone. This makes it a rather
speaking. It carries the virtue of "inter

Mode 10. Extreme Change of All Fo
we have the mode that makes variation
variation may be predicated *a priori* as
then this ought to prove an impressiv
manner. Every kind of change is emplo
No one kind of change or lack of chan
any length of time. While great variety
yet only those changes were chosen whic
were in conformity with the social spee
of auditors who served for subjects.
empirically would be judged effective
place statements interesting and easy to

Mode 11. No Change in Any of the F
manner involved complete uniformity of
and quality—the extreme of monotony.
prehension of the logical meanings of
all syllables are given the same durati
tensity, in addition to monotony of p
change of quality, the listeners seem
catching the words. Yet this applies
the reading more than later.

THE READING

The reader made no attempt to add
of his reading by any other means than
No gestures, postures, or facial expres
all such devices for inducing response
and successfully, it is fair to say, elimi
fortably in his chair, most of the time
occasionally leaning forward on the de
read in the manner he had prescribed f

At the close of the reading the reader shut the book, and announced that that was all for that reading. Then an interval of a few minutes was allowed, varying from two minutes to five. During this interim the experimenter wrote on the blackboard the title of the second story to be read during the next half of the period, together with the names of the characters that appeared in that story. The auditors were free to move about the room or to remain in their seats. The only prohibition put upon them was that they must not talk about the story just read or about the characters in it or about stories of which it reminded them. In fact they were prevented from making any reference or allusion to it that might establish or strengthen associations.

At the close of the second reading, the experimenter passed out the papers on which the reports were to be written. These papers were folded, numbered on the back, and dated with the date on which they were to be filled in. Subjects were instructed not to unfold them, and under no circumstances to look at the contents until ready to write out their report. The papers were presented in such a way that there was no occasion for looking at their contents. There is every reason to believe that this instruction was carried out to the letter. When the papers had been given out, all the auditors left the room.

Length of Reading. The reader stopped arbitrarily at the close of the twenty minutes. With two readings a week for each subject and with two readings a meeting, a large number of stories had a place in the experiment.

The reports were returned to the experimenter any time after the fifth day.

Reports

Purpose. The purpose of the reports is to provide a measure of impressiveness of the different modes of reading, and to give this measure in mathematical terms. To do this required a new method with very little precedent as guide.

Tentative Method of Completion Test. In the first test to measure retentiveness of impression according to modes of reading, the subject was merely asked to write down what he remembered. No restrictions were placed upon the amount to be written, whether a brief summary or an extended narrative. As a consequence the reports differed so widely in the number of words employed and in minuteness of detail that they were not at all comparable.

Method Revised Further. To remedy th
of instructions was given the subjects, as

1. Identify and characterize the differen
2. Describe briefly the places pictured.
3. Give a consecutive account of the eve
4. Outline the conversations given.

Again the differences in the fulness
subjects were too great to make the result
some subjects wrote volubly on these p
most laconic manner possible. So this dev

Method Finally Chosen. To remedy thi
of "leads" was adopted. The "lead" i
plete declarative sentence so selected th
observer some situation incident, or fact c
When the sentence is incomplete, then
parentheses some instruction as to what to
sentence. If the sentence is complete, h
parentheses is given some instruction as
details, consequences, or characterizatio
sample of the type of "leads" used in th

THE INN

The inn of which we speak possesses many
The present proprietor is Naum (describe hi
The previous proprietor had been Akim (des
Akim kept the inn (how?).
The land belongs to Elizaveta Kuntz (charact
Relate the affair between Akim and Dunyash

Another example of this type of "lead":

FATHERS AND CHILDR

The governor in the narrative was to be inv
Matyei, sent to investigate, was (characterize
Matyei stunned his subordinates in the char
(how?).
Sitnikoff the dilettanti took them to the hom
(describe her).
The present domestic status of Madame K. w
The three men were induced to go by (what
Madame Kukshin talked of (detail her interest

LIEUTENANT ERGUNOF

The Lieutenant was treated by the ladies fro
specific details).
One day the Lieutenant, wearied by (what?)
what results?).
The next day the Lieutenant received a letter
Upon his arrival next day (describe his welco
(Then what happened? Relate in detail.)
(Give a visual and an auditory picture of t

Leads

Some Problems Involved. In devising leads several considerations have to be kept in mind, in order to make the data comparable:

(1) Nothing subjective was asked for beyond interpretations as to character and the meaning of events in the story. In the preliminary stages of the use of this method occasional descriptions and characterizations were demanded; but results demonstrated that individual differences in responding to such leads were so great that the results of the various subjects could not be compared.

(2) The matter asked for in the leads was uniformly concrete.

(3) Leading questions, those that can be answered with Yes, or No, were entirely eliminated. Also questions were so devised as to make difficult the inferring of the answer from the nature of the question.

(4) The leads were chosen in such a way that the total effect of them was to produce a running summary of the story in its chief details.

Number of Leads. According to this method there is no fixed number of leads for a report. In fact, the limiting of the number of leads to some specified figure would bring an error into the records, for the reason that there are differences in style from one story to another of enough significance to make necessary a flexible joint in the method. Thus the choosing of the number of leads is entirely a matter determined by the nature of each story or passage. Especially determinative of the flexibility is the matter of concreteness in the passage. In particular, events in time make serviceable material for leads. The number of leads presented on reports differs from 8 to 35.

Scoring

Determining the Value of a Point. In general every recorded perception of a process was counted a point. Then when characterizations were demanded, if the number of these was specified, the fixing of the value of a point was a function of the number demanded; if the number was not called for, the experimenter had to decide what kind of answer constituted a point. In general a "point" is [illegible] defined here as a recorded fact of an event, [illegible] characterization, [illegible] a description. As nothing but objective matter was asked

the factual basis of points made scoring reports reasonably accurate and uniform.

Specific Answer. The answer to the lead must satisfy the specific question involved before any credit at all is given.

No Overlapping. The number of points given any specified lead must be only such as are needed to tell the facts called for without *overlapping*.

Essential Details. Only such details as are essential to the event, characterization, or description are accepted.

Verbosity vs. Conciseness. Neither verbosity nor conciseness are determining factors in scoring points. Some reporters are habitually wordy, others are by habit laconic. Regard must be had for this difference in estimating the number of points to be scored. A single word can carry the evidence that the reporter has retained the essential fact while sentences may be employed by another reporter without adding to the evidence for retention. This was kept in mind consistently in making out the scores.

Lack of Specificity in Answers. No credit is given for statements that do not specifically answer the question asked.

Guesses. Subjects are warned against offering guesses; they are encouraged to leave blanks rather than make replies the correctness of which they cannot substantiate. The important consideration in the understanding of their task is that they are not to make guessing a practice for the sake of shielding themselves from the effort necessary to answer with assurance, or for the sake of a full report.

The Total Score. The total score for a report is a fraction which uses the whole number of points assigned to that report for a denominator and the total number of points scored for a numerator. For purposes of uniformity this fraction is treated in the results on a percentage basis.

Mechanism of Scoring. As a measure for stabilizing the judgment of the scorer, all scoring was done with the reports of one selected painstaking subject as guide.

The first step in scoring is to estimate each lead separately, to determine how many points are needed to represent full retention for this lead. The number thus secured is then placed beside the lead at the left side of the paper. After all of the leads have been thus estimated, the total is found and set down at the top of the sheet for the denominator. The score of the paper, however, is not yet settled finally; two or three more papers are graded on this basis before arriving at a final judgment as to the weight of each lead and

so of the size of the denominator. This reading of additional reports sometimes shows errors in the preliminary judgment, or reveals that the replies in the report of the guide auditor are not precisely representative. Revision is then made of those thus far graded, and with the revised denominator as a final judgment the scores are made out for all the reports and entered at the top of each sheet both as a fraction and as a decimal.

A TYPICAL REPORT GRADED

Reading A2

Wide Range of Pitch

MUMU

(Vol. 2, to p. 218)

(2) IN MOSCOW LIVED WITH her retainers and
2 a Russian mistress (widow) servants

(5) AMONG THEM WAS GERASIM WHOSE DUTIES WERE (SPECIFY FULLY)
3 to sweep out the courtyard/fetch water/and generally guard the premises/

(3) GERASIM HAD PROVED HIS COMPETENCE BY (SPECIFY THREE DEEDS)
3 not letting any dirt appear in the yard/if the cart for fetching water stuck in the mud he extricated it by his own strength/knocking the heads of two hapless robbers together/

(4) IN CONVERSATION WITH GAVRILA THE MISTRESS DECIDED TO
3 marry off one of her dependents, a drunkard/in the hope of reforming him/and settle on Tatyana as his destined wife/

(3) GERASIM'S COURTSHIP OF TATYANA HAD CONSISTED OF (3 ITEMS)
2 presenting her with little tokens as she passed him in the courtyard/not permitting [illegible] er to be made sport o[illegible] in his presence/

(2) THE MISTRESS UPO[illegible] [illegible] OURTSHIP WAS
[illegible] did not think the M[illegible]
[illegible]im's courtsh[illegible]

(3) KAPITON'S REPLY TO GAVRILA CONCERNING TATYANA WAS

3 that he would undoubtedly be killed by Gerasim/but that he was always doomed to ill luck/and might as well marry. He liked Tatyana herself/

(2) THE EFFECT OF THE COURTSHIP UPON TATYANA WAS

2 merely to frighten her to death/and to acquiesce in the courtship for that reason/

Score—18/24; .75

Significance of Symbols. The capital letters used as designations of the readings represent the mode in which the reading is done. (pp. 126-7.) The Arabic numerals used in connection with the capital letter represent readings 1 and 2, the first and the second respectively during the hour of meeting. The text in capital letters represents the leads given out on the reports; the rest of the text is the answers given by the subjects. The Arabic numerals in parentheses at the left represent the total number of points allowed to the lead beside which they are placed; the numerals not in parentheses are the score given to the answer. The fraction at the bottom of the report represents the total score, which is also given as a decimal. The use of the bar (/) in the answers marks the division into points; they are inserted to indicate the basis of marking off into points.

Preliminary Discussion

Constant Factors. The attempt was made in this experiment to keep the conditions as similar as possible to the conditions which prevail in places of public meeting. The typical "audience" was of three, occasionally a fourth being present, and at rare intervals five. Audiences of two were very few in the total number of tests.

Thus the social environment of an audience was achieved in all cases. The tests were held in Room 27, Emerson Hall, Harvard University. Except when the hour was the first in the morning and subjects showed a tendency to straggle in as when going to public places, they gathered in the corridor outside the room; they were instructed not to enter until the whole group had gathered, or to take their seats and comport themselves in the manner usual to them in going into a place of public meeting. Then as soon as all were in their places, the experimenter took charge of the meeting and directed the activities of the subjects from the platform.

Instructions. The experimenter then read the following instructions:

1. "Attend as you are in the habit of attending at public gatherings.
2. "At the close of the hours you will pass out without being called upon for a report or discussion of the content of the reading.
3. " On the fifth day after the reading, and not before or later, you are to fill out a blank prepared by the experimenter, which will be placed in your hands at the close of the hour.
4. " No feats of memory are sought, and no penalty or reward attaches to the amount you remember; merely set down at the time of reporting what you have retained.
5. " Keep the conditions of reporting as uniform as possible; use the same time of day, the same working surroundings, the same place, the same amount of time, the same degree of care, and the same degree of effort to recall."

During the early tests these instructions were repeated at the beginning of each period; later, paraphrases were given carrying the same implications; while still later nothing more was said as to instructions other than to remind the company that the instructions were the same as before. The subjects were repeatedly instructed that they were to listen as they are in the habit of listening when out in a place of public meeting, and that they were not called upon for feats of memory; they were *merely to listen as they do usually,* and to return an honest report written under circumstances uniformly the same.

The Choice of Reading Matter. The choice of reading matter for this kind of experiment offers several nice problems. In the first place, the matter must be unfamiliar to the auditors; secondly, the reading matter must be of a type to hold interest uniformly keen among the whole group of subjects—narration, and narration that is stimulating, with definite type of characters, and written by a writer conceded widely to be a master of style. The higher the degree of uniformity in certain essential particulars, the more valuable is the matter for the present purpose. Uniformity of rhetorical style, strength of characterization, plot structure, descriptive power, and structural strength, are kept at a maximum by adherence to the works of the same writer. Then, again, uniformity of types of people presented in the stories is an aid to general uniformity of type of matter; also similarity of situation, geographical surroundings, the manner of living of people, and national setting.

With all these requirements in mind, the works chosen for this experiment were the novels and stories of the Russian writer, Turgenieff.[5] Inquiry revealed that the auditors assigned for this experiment had read practically none of his writings. From the first tests it became manifest that the element of interest was adequately provided, judging by the attitudes of the listeners during the reading and by their comment afterwards. Some had to be specifically instructed not to read the author's works during the course of the experiment; they had asked permission to read some of his stories by themselves.

Proper Names. The matter of Russian names was handled in the following manner. At the beginning of the meeting, as soon as the audiences had taken their places and the reader had taken the chair, the title of the story was placed on the blackboard before the audience, together with the names of the characters who participated in the story. This was done with a view to reducing to a minimum any confusion over names so unfamiliar to an American audience as are the names of the Russian people. In no way throughout the experiment was any attempt made to require subjects to remember these names; while every effort was exerted to make recognition of them easy. By putting the names upon the blackboard a visual perception was added to an auditory. Always in the reports these names were so presented by the experimenter that the auditor never had to recall the name in order to indicate which of the people of the story he was discussing or reporting on. How this was handled will appear in the transcript of forms of report presented above.

Resumé of Previous Readings. After the names of the characters of the story had been written upon the blackboard, the reader announced, "I am now going to read you part of a story by Turgenieff, entitled.................." When the story was to be used for the first time, nothing more than this was said. However, when part of the story had been read to a given group, the reader preceded the test with a brief resumé of the story up to the point at which he proposed to begin. This was done to enable every subject to start out with as full an equipment as possible of the story up to that point, so that those of superior retentiveness would not start with an advantage over those who happened to have retained little from the reading preceding. In this way a source of error was eliminated.

[5] The edition used is the translation by Isabel F. Hapgood, and published in 7 volumes by Charles Scribner & Sons, New York, 1915.

TABLE I: RECORDS OF IMPRESSIVENESS—SERIES I

20-minute Readings; 2 Readings per Session
Reported after 5-days interval
Auditors Unadapted to Experiment

Modes of Reading (See pp. 15–21)	Subjects 1		11		15		21		22		23		24		25		26		27		
	a	b	a	b	a	b	a	b	a	b	a	b	a	b	a	b	a	b	a	b	Av.
1	.80	.70	.24	.04	.28	.04	.00	.08	.32	1.00	.08	.19	.32	.70	.60	.81	.92	.52	.60	.63	.45
2	.89	.65	.48	.17	.00	.30	.48	.26	.70	.70	.56	.35	.56	.61	.85	.78	.85	.65	.89	.91	.58
3	.94	.80	.00	.31	.00	.19	.13	.31	.75	.65	.00	.19	.29	.50	.54	.15	.58	.59	.63	.39	.39
4	.94	1.00	.11	.09	.72	.64	.33	.32	.95	.96	.06	.23	.83	.86	.61	.86	.44	.96	.56	.68	.60
5	.83	.88	.60	.00	.88	.83	.13	.00	.93	.88	.13	.53	.57	.82	.57	.88	.75	.82	.78	.41	.61
6	.81	.93	.00	.14	.81	.64	.00	.14	.75	.93	.19	.29	.56	.43	.75	.79	.88	.64	.81	.71	.54
7	.92	.87	.00	.57	.00	.57	.00	.00	1.00	.48	.18	.35	.00	.43	.55	.91	1.00	.87	.00	.57	.47
8	.64	.63	.00	.13	.00	.06	.00	.00	1.00	.83	.00	.19	.91	1.00	.81	.94	.73	.56	.00	.19	.43
9	1.00	.88	.00	.00	.64	.00	.45	.29	.82	.82	.00	.06	.64	.71	.73	.65	.45	.71	.64	.35	.70
10	1.00	1.00	.38	.00	.69	.62	.38	.38	1.00	1.00	.38	.00	1.00	.83	.88	.92	.88	.83	.88	.92	.70
11	.63	.60	.08	.00	.15	.00	.15	.00	.21	.87	.05	.00	.00	.60	.42	.40	.16	.67	.16	.47	.28
Av.	.83		.75		.37		.17		.79		.18		.55		.69		.66		.55		

GRAPH 1:—FROM TABLE I—SERIES I—COMPARATIVE IMPRESSIVENESS OF VARIOUS MODES OF READING

TWENTY MINUTE PASSAGES; REPORTED AFTER FIVE DAYS

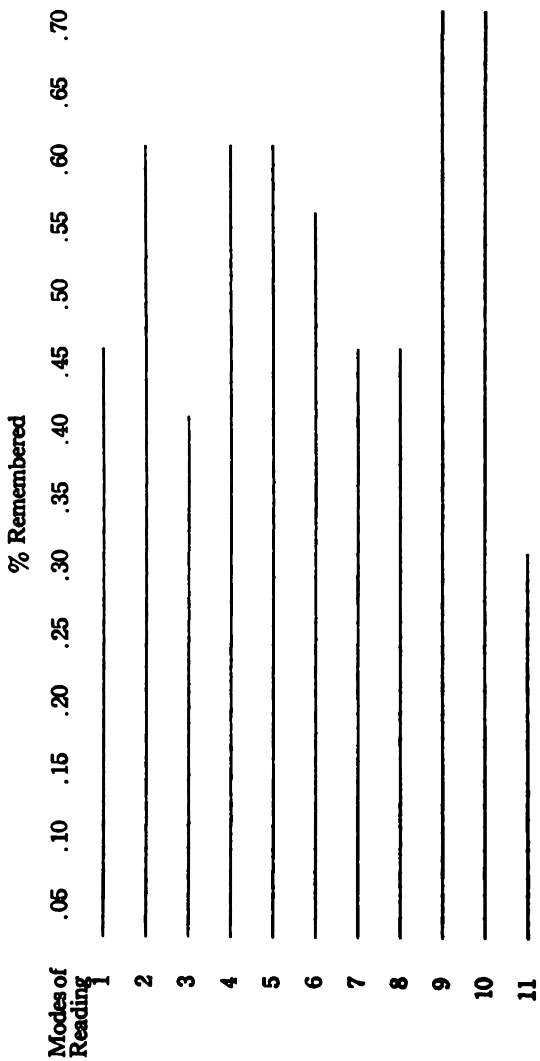


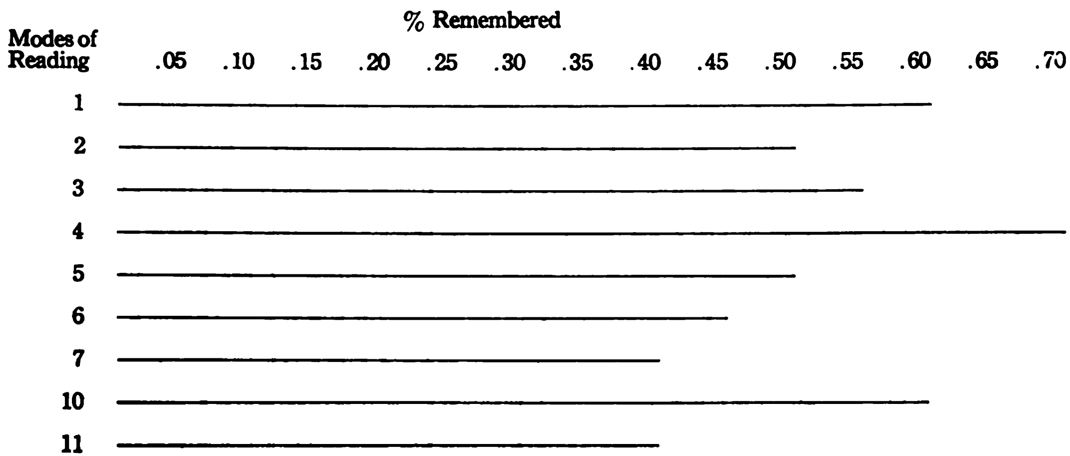
TABLE II: RECORDS OF IMPRESSIVENESS—SERIES II

TWENTY MINUTE READINGS—TWO READINGS PER SESSION REPORTED AFTER FIVE DAYS INTERVAL. AUDITORS ADAPTED TO EXPERIMENT

Modes of Reading (See pp. 15–21)	Subjects 1		11		15		21		22		23		24		25		26		27		
	a	b	a	b	a	b	a	b	a	b	a	b	a	b	a	b	a	b	a	b	Av.
1	1.00	1.00	.12	.29	.29	.76	.11	.48	.76	.52	.00	.38	.82	.86	.76	.76	.59	.86	.59	.76	.61
2	1.00	.88	.00	.00	.00	.88	.67	.00	.62	.63	.00	.00	.50	.94	.33	.69	.67	.79	.25	.69	.49
3	1.00	.95	.78	.60	.61	.65	.30	.00	.65	.70	.00	.40	.70	.80	.74	.65	.91	.65	.52	.17	.58
4	.84	.87	.82	.70	.83	.87	.23	.00	.63	.70	.20	.57	.82	.74	.97	.78	.60	.87	.83	.83	.69
5	1.00	.78	.35	.56	.29	.61	.12	.00	.79	.67	.12	.00	.65	.50	.76	.61	.65	.56	.65	.39	.48
6	.80	.50	.10	.21	.60	.50	.00	.00	.70	.38	.00	.04	.60	.46	.85	.63	.65	.58	.65	.71	.45
7	1.00	.94	.10	.17	.40	.28	.00	.39	.60	.89	.00	.11	.35	.50	.60	.50	.65	.83	.35	.17	.43
10	1.00	1.00	.42	.00	.31	.86	.00	.00	.58	1.00	.15	.14	.81	.62	1.00	.76	.95	.95	.85	.86	.61
11	.65	.70	.29	.35	.71	.15	.00	.00	.65	.50	.00	.00	.76	.35	.24	.35	.41	.70	.65	.60	.41
Av.	.82		.28		.53		.13		.66		.13		.63		.68		.72		.58		

GRAPH 2—FROM TABLE II—SERIES II—COMPARATIVE IMPRESSIVENESS OF VARIOUS MODES OF READING

TWENTY MINUTE PASSAGES. REPORTED AFTER FIVE DAYS



LIST OF ADOLESCENT-ADULT POINT-SCALE TESTS WITH MODE OF PRESENTATION AND TIME ALLOWANCE IN GROUP EXAMINING

1. Response to pictures	Lantern slides	3 minutes for each picture
2. Comparison of weights	Cubes handed to students	No limit
3. Memory span for digits	Spoken by examiner, two per second	No limit
4. Suggestibility	Lantern slides	10 seconds for each question[2]
5. Memory for unrelated sentences	Spoken by examiner	Time necessary for slowest writer
6. Comparison of terms	Spoken by examiner	1 minute + for each pair of terms
7. Comprehension of questions	Spoken by examiner	1 minute + for each question
8. Definition of terms	Spoken by examiner	1 minute + for each term
9. Appreciation of questions (absurdities)	Spoken by examiner	1 minute + for each
10. Analogies	Spoken by examiner	30 seconds for each analogy
11. Association of opposites	Spoken by examiner	No time fixed
12. Relational test	Lantern slides	2 minutes for each part
13. Box test	Spoken by examiner	1 minute for each part
14. Ingenuity test	Spoken by examiner	3 minutes for each part
15. Comparison of capital letters	On record blank	2 minutes for test
16. Code learning test	Lantern slides	20 seconds for learning[2] and 2 minutes for reproducing symbols of each part
17. Ball and field	On record sheet	3 minutes for test
18. Geometrical construction	Lantern slide	3 minutes for 1st trial; 2 minutes for 2d
19. Reproduction of diamonds	Lantern slide	1 minute for reproduction
20. Memory for designs	Lantern slide	10 seconds exposure;[2] 2 minutes for reproduction

records, as well as those of a few foreign students who could not readily understand English, were omitted. The men included representatives of the four collegiate classes, the sophomore and junior classes being most

[2] Two seconds additional were allowed for visual adjustment.

largely represented. The women were all of the senior class. Of the men, the average age was 21.16 years; of the women, 22.2 years.

STATISTICAL RESULTS OF POINT-SCALE EXAMINATION

The principal results of the application of the point scale to these two groups appear in the accompanying Table I. In addition to the mean score for each of the twenty tests, there is presented for each group the relation of the mean to the maximum score, the mean variability, and the percentage mean variability. The last column on the right indicates the relations of the scores for the sex groups. For

TABLE I

Statistical Results of Point-Scale Examinations

Test	Men (100)				Women (119)				Men Compared with Women, Per Cent.
	Mean	Per Cent. of Maximum	M. V.	Per Cent. M. V.	Mean	Per Cent. of Maximum	M. V.	Per Cent. M. V.	
1	5.55	61.7	1.40	25.2	5.52	61.4	.31	5.6	+ .3
2	2.92	97.4	.147	5.0	2.98	99.4	.034	1.1	— 2.0
3	4.05	81.0	.82	20.2	3.47	69.4	.995	28.6	+11.6
4	3.30	66.0	.986	29.8	3.71	75.6	1.14	30.8	— 9.6
5	4.32	86.4	.64	14.8	4.61	92.2	.538	11.7	— 5.8
6	4.85	80.9	.96	19.8	4.85	80.9	.89	18.3	0
7	4.22	84.5	.67	15.8	4.05	81.0	.614	15.2	+ 3.5
8	4.52	56.5	.308	6.8	4.86	60.7	1.25	25.7	— 4.2
9	4.98	99.6	.039	0.8	4.95	99.0	.064	1.3	+ .6
10	5.60	93.3	.566	10.1	5.57	92.9	.53	9.5	+ .4
11	3.85	96.3	.256	6.6	3.89	97.0	.202	5.2	— .7
12	6.98	87.3	1.00	14.3	6.45	80.6	.799	12.3	+ 6.7
13	3.94	98.5	.116	2.9	3.64	91.0	.58	15.9	+ 7.5
14	4.02	57.4	2.26	56.2	1.38	19.7	1.82	132.0	+37.7
15	1.86	93.0	.25	13.5	1.39	69.5	.782	56.3	+23.5
16	5.18	86.4	1.11	21.5	4.81	80.2	1.38	28.6	+ 6.2
17	1.60	80.0	.616	38.5	1.45	72.5	.773	53.3	+ 7.5
18	2.58	64.5	1.50	58.2	2.18	54.5	1.63	75.0	+10.0
19	1.92	96.0	.148	7.7	1.89	94.5	.202	10.7	+ 1.5
20	3.82	95.5	.32	8.4	3.66	91.5	.454	12.4	+ 4.0

Data for Entire Scale

Mean	M. V.	% M. V.	Mean	M. V.	% M. V.
81.06	5.78	7.13	75.235	5.48	7.15

example, test 1, response to pictures, yielded the following statistical data: (1) for men (*a*) mean, 5.55; (*b*) percentage of maximum score, 61.7; (*c*) mean variability, 1.40; (*d*) percentage mean variability, 25.2; (2) for women (*a*) mean, 5.52; (*b*) percentage of maximum score, 61.4; (*c*) mean variability, .31; (*d*) percentage mean variability, 5.6; (3) score for men .3 per cent. more than that for women.

Chiefly significant in this table are the following facts: The mean score for the men, 81.06, exceeds that for the women, 75.235, by 5.825 points. The probable error of this difference in the sex scores is 0.647. This sex difference is obviously significant, since it is approximately nine times the probable error. The variability of results, as expressed in terms of per cent. of the mean, is practically the same for the two groups. Comparison of the group results for the individual tests indicates that the first half of the point scale, which is constituted by tests that depend chiefly upon observation, memory, imagination and simple ideational processes, reveals no constant sex difference. In certain of the tests, the men are markedly superior; in others, the women. For example, in test 3, memory span for digits, the score for the men exceeds that for the women by 11.6 per cent.; whereas, for test 5, memory for unrelated sentences, the women's score exceeds the men's by 5.8 per cent. Contrary to expectation, the measurement of suggestibility, test 4, shows a superiority of the women over the men amounting to 9.6 per cent. By contrast, the latter half of the scale yields results which are strongly in favor of the men. The tests are predominantly of the ideational sort, and several of them involve complex thought processes. In practically all of the tests which may be described as measurements of reasoning, the men are superior to the women. Thus,

in test 14, Terman ingenuity test, the score for the men exceeds that for the women by 37.7 per cent.; in test 15, comparison of capital letters, by 23.5 per cent.; in test 18, geometrical construction, by 10 per cent.

These sex differences are so numerous and so great as to compel attention and to justify the tentative conclusion that sex norms should be rendered available and used in connection with many of our mental measurements.

The distribution of the point-scale scores for the two collegiate groups appears in Table II., ten classes having been employed for this purpose.

TABLE II

Distribution of Point-Scale Scores

Scores	Men	Women
50–54 points	———	1 (0.9%)
55–59 points	———	1 (0.9%)
60–64 points	———	8 (6.7%)
65–69 points	6 (6%)	11 (9.2%)
70–74 points	11 (11%)	27 (22.6%)
75–79 points	25 (25%)	40 (33.6%)
80–84 points	23 (23%)	23 (19.3%)
85–89 points	23 (23%)	8 (6.7%)
90–94 points	11 (11%)	
95–100 points	1 (1%)	

We have calculated the coefficients of intelligence for the individuals of the groups[3] by determining the ratio of the individual scores to the mean for the appropriate group. Thus, the coefficient of intelligence for a man who scored 81 points would be 81/81, or otherwise expressed, 1.00; that for a woman who scored the same number of points would be 81/75.23, or 1.08—.

[3] The coefficient of intelligence is the ratio of the individual point-scale score to the expected score or norm. See article by Yerkes and Wood on "Methods of Expressing Results of Measurements of Intelligence: the Coefficient of Intelligence," *Journal of Educational Psychology*, 1916, 7, 593–606.

These coefficients have been classified below in accordance with a method suggested by Yerkes and Wood.[4] Only three classes are represented, the subnormal, the normal, and the supernormal. Individuals obtaining coefficients of intelligence ranging from .71 to .90 are designated as subnormal; those whose coefficients range from .91 to 1.10, as normal; those with coefficients from 1.11 to 1.30, as supernormal.

Table III. indicates the number and the percentage of individuals in each of these categories, and the individual coefficients for men and women of the subnormal and the supernormal classes.

TABLE III

Distribution of Coefficients of Intelligence

Intelligence Class	Men	Women
Subnormal:		
(coefficients .71– .90) ..	12 (12%)	19 (16%)
Normal:		
(coefficients .91–1.10) ..	76 (76%)	87 (73%)
Supernormal:		
(coefficients 1.11–1.30) ..	12 (12%)	13 (11%)

INDIVIDUAL COEFFICIENTS

Subnormal Class		Supernormal Class	
Men	Women	Men	Women
.89 (4)	.90 (4)	1.17	1.18
.88	.89 (3)	1.15	1.17 (2)
.86	.88 (2)	1.13 (3)	1.16 (3)
.85 (2)	.85 (2)	1.12 (2)	1.14 (2)
.83	.84	1.11 (5)	1.13
.81 (2)	.82 (3)		1.12
.80	.81		
	.80		
	.77		
	.70[5]		

The women obtained the highest (1.18) as well as the lowest (.70) coefficients of intelligence. The highest for the men is 1.17; the lowest, .80.

[4] *Journal of Educational Psychology*, 7, p. 600.

[5] Strictly, this coefficient belongs in the class "intellectual inferiors."

It is probable that few of the individuals who were classed as of subnormal intelligence and whose coefficients range from .71 to .90 are capable of profiting in any large measure by collegiate training. The writers venture the prediction that thorough psychological examination of these individuals would furnish convincing evidence that at least half of them (6 to 8 per cent. of the groups) should be excluded from the college for their own welfare.

CORRELATION OF POINT-SCALE SCORES WITH COLLEGIATE GRADES

The men were given letter, instead of percentage, grades in their course in psychology, on the basis of numerous tests written in class. The course may fairly be described as a difficult one for beginners in the study of psychology. There is every reason to suppose that much more was expected of the students than is the rule in elementary courses, and that every student was forced to exert himself fairly steadily in order to attain a passing grade. For the purpose of correlation, the final grades in this course have been thrown into three groups. That which may be designated as the first tertile consists of grades *A* and *B*; the second tertile, of grade *C*; and the third tertile, of grades *D* and *E*. In these three groups there appear, respectively, twenty-one, fifty and twenty-five students.[6]

Similarly, the point-scale scores have been thrown into three groups. The first or highest tertile includes all scores from 88 to 93 points; the second tertile, those from 76 to 87 points; the third tertile, those from 65 to 75 points.

Below appear the percentages of individuals

[6] Four students failed to complete the work of the course by taking the final examination. Consequently, no grade has been assigned them.

appearing in each pair of tertiles. The positive correlation is obvious, since 43 per cent. of the members of the class appear in the first tertile with respect both to intelligence and to performance in psychology, whereas only 8 per cent. of the students who received grades of *D* or *E* obtained point-scale scores between 88 and 93 points.

TABLE IV

Correlation of Point-Scale Measurements with Measurements of Educational Performance

Men

	88–93 Points 1st Tertile Point Scale (20 Students)	75–87 Points 2d Tertile Point Scale (54 Students)	66–75 Points 3d Tertile Point Scale (22 Students)
A and *B* 1st tertile (Psych. I.) (21 students)	43%	52%	5%
C 2d tertile (Psych. I.) (50 students)	20%	56%	24%
D and *E* 3d tertile (Psych. I.) (25 students)	8%	56%	36%

Women

	81–89 Points 1st Quartile Point Scale	77–80 Points 2d Quartile Point Scale	72–76 Points 3d Quartile Point Scale	53–71 Points 4th Quartile Point Scale
1st quartile college (28 students)	32%	29%	21%	18%
2d quartile college (25 students)	20%	16%	32%	32%
3d quartile college (28 students)	13%	29%	29%	29%
4th quartile college (26 students)	27%	23%	19%	31%

The degree of positive correlation between point-scale scores and grades in psychology for the men indicated by our figures is higher than has usually been obtained by other investigators.[7] Our only suggestion, by way of

[7] See, for instance, Hollingworth, H. L., "Vocational Psychology," Appleton, 1916, p. 166; Bell,

explanation, is that the students were held strictly to account throughout their course, having been graded frequently and carefully on the basis of written work and having presumably been held fairly close to the maximum of their intellectual ability.

The correlation between measurements of intelligence and educational performance for the women is indicated by a method different from that used above. The standing of each individual was determined by dividing the total number of credits for courses passed with a grade of *C* or better by the number of such credits that should have been obtained. Thus there was made available a grade for the first three years of collegiate work of each student. This grade presumably fairly represents the educational performance of the individual during that interval. The grades thus obtained are distributed in four groups, designated, respectively, as the first, second, third and fourth quartile. In these groups there appear twenty-eight, twenty-five, twenty-eight and twenty-six students.

Similarly, the point-scale scores for the women are divided into four groups, the first quartile containing all scores between 81 and 89 points; the second, all between 77 and 80 points; the third, all between 72 and 76 points; and the fourth, all between 53 and 71 points.

The correlation of mental and educational measurements for the women, although positive, is much less markedly so than for the men. This is due, so far as we can tell, chiefly

J. C., "Mental Tests and College Freshmen," *Journal Educational Psychology*, 1916, 7, 381 (Bell quotes Wissler's and Waugh's coefficients); Whipple, G. M., "Manual of Mental and Physical Tests," Baltimore, 1915; Brown, William, "The Essentials of Mental Measurement," Cambridge, 1911, pp. 112 ff.; Münsterberg, H., "The Freshman Studies," *The Harvard Illustrated Magazine*, 1916, 17, 170.

to the fact that the grading for the course in psychology taken by the men was closer and in every way more rigorous than that for the courses which determined the standing of the women. Our individual results seem to indicate that in the women's group there are many students of fairly good intelligence who because of laziness or carelessness obtain poor grades in their collegiate courses.

CONCLUSIONS

1. We are justified in concluding *for the groups in question* that the college men, with respect to the majority of intellectual functions measured by the point-scale method, rank higher than the college women.

2. That the superiority of the men is especially marked in tests which involve reasoning or other fairly complex thought processes, while the sex differences are least for tests of perception, memory and imagination.

3. That a slightly higher percentage (16 per cent.) of the women are of subnormal intelligence than of the men (12 per cent.).

4. That the correlation of point-scale measurements with educational performance is strikingly positive for the group of men observed and somewhat less positive for the group of women.

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